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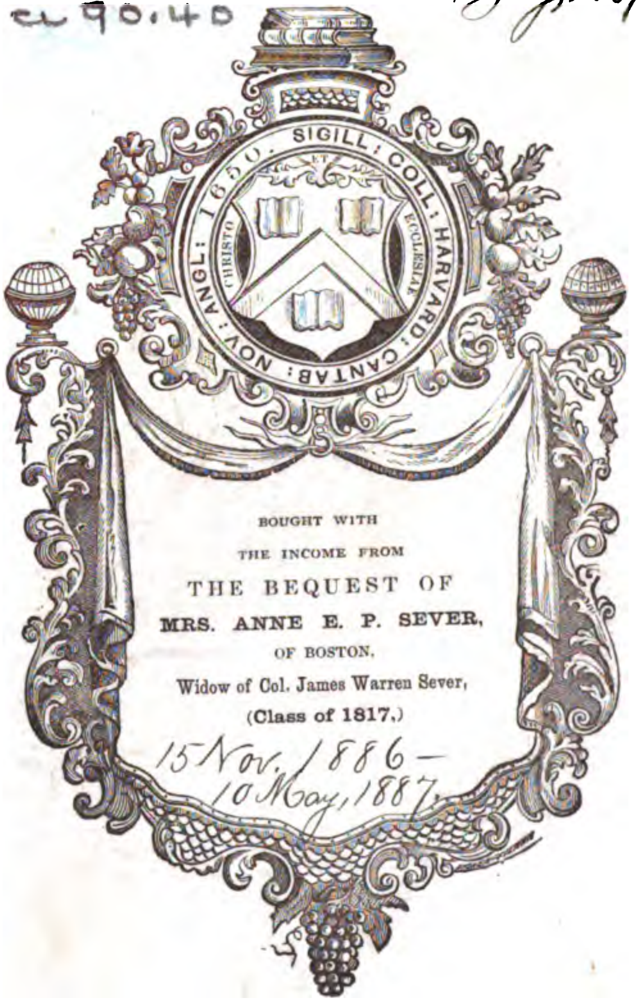
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25-425

Nature

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

0

Nature

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XXXV

NOVEMBER 1886 to APRIL 1887

" To the solid ground
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London and New York
MACMILLAN AND CO.
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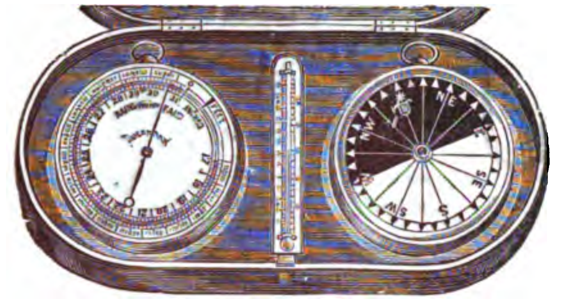

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 4, 1886

EXPLOSIONS IN COAL-MINES

Explosions in Coal-Mines. By W. N. and J. B. Atkinson, H.M. Inspectors of Mines. (London: Longmans, 1886.)

EVERYBODY in the least degree conversant with matters connected with coal-mining will at once admit that our knowledge of the remote causes of colliery explosions has increased enormously during the last few years. Whether, however, the practical application of this knowledge has kept pace with the rate of increase in the knowledge itself is another matter. Since 1851, when the first Mines Inspection Act was in force, the number of fatal explosions in collieries has steadily diminished, but the annual loss of life from these catastrophes is as great as ever. During the ten years ending 1860 there were 820 fatal explosions, resulting in 2441 deaths, or an average of 2·98 deaths per fatal explosion; during this decade there was an average of 3000 persons employed in and about the mines for every fatal explosion, and 1008 persons for each resulting death. During the ten years ending 1870 the number of fatal explosions fell to 565; the deaths were 2267, or an average of 4·01 per fatal explosion; and the ratio of persons employed to each fatal explosion was 5650, and hence to each resulting death 1408. During the ten years ending 1880 the number of fatal explosions was 424; the resulting deaths were 2686, or an average of 6·33 per fatal explosion; the ratio of persons employed to each fatal explosion was 11,372, and to each resulting death 1795. During the five years ending 1885 we have had 146 fatal explosions, with a loss of 906 persons, or an average of 6·20 deaths from each explosion; the ratio of persons employed to each fatal explosion was 17,503, and to each resulting death 2820. These figures are in the highest degree significant, but they are not capable of telling everything. They do not, for example, bring out the fact that the actual violence of colliery explosions when they do occur is nowadays greater than formerly. This may seem to be indicated by the increase in the average number of deaths from each fatal explosion, but then, on the other hand, there are far more men employed in pits now than formerly. The diminished number of explosions is probably due, in the first instance, to the more general employment of safety-lamps, and, during late years, to the restrictions which have been placed upon the use of explosives. The increase in the average number of deaths to each explosion is doubtless owing to the gradual deepening of the pits and to differences in the mode of origin and character of the explosion. Thirty years ago the pits as a rule were comparatively shallow and damp. Such a sinking as that of the Ashton Moss pit at Audenshaw, which is upwards of half a mile deep, was unknown. Explosions in these damp shallow pits were usually caused by the ignition of gas, most frequently by naked lights; they were very local in their action, and the loss of life was small. Nowadays an explosion in a deep and dry mine not unfrequently penetrates throughout the whole pit; it is often extremely violent, and the number of deaths, mainly from after-damp, is correspondingly great.

There can be very little doubt that such explosions are, in the main, caused by dust. The fact that fire-damp is not the only explosive agent which may be present in coal-mines is now generally recognised. It is, however, a moot point with many practical men whether coal-dust alone, in the entire absence of gas, can bring about an explosion of any magnitude. It is generally conceded that a very small amount of gas, an amount, indeed, too small to be recognised by the elongation of the flame of a safety-lamp, or the formation of a "cap," is sufficient in the presence of coal-dust to form a dangerously explosive atmosphere, but colliery managers and many mining engineers have, apparently, been slow to believe that dust itself may, under certain conditions, effect an explosion quite as violent in its character as the most formidable gas explosion of which we have any record. The Royal Commissioners appointed to inquire into accidents in mines reported that in their opinion it was well established that even when the air is quite free from fire-damp, an exceptionally inflammable coal-dust, in a very finely-divided and dry condition, and existing in abundance in the immediate vicinity of a blown-out shot, may when raised by the shot be ignited so readily and carry on the flame so rapidly that it may produce explosive effects of a similar character to those caused by a gas explosion. The flame as it rushes along, if fed by freshly raised dust, may extend under these circumstances to very considerable distances, with results resembling, in their disastrous nature, those of explosions originating with, and mainly due to, fire-damp. This conclusion is very greatly strengthened by the evidence which the Messrs. Atkinson have brought together in the book before us. Their work indeed constitutes the most formidable indictment against coal-dust as a cause of colliery explosions which has yet been drawn up. In their capacity of Inspectors they have investigated with the most patient care the circumstances connected with what we may call six typical explosions. These were—

Date 1880	Name of colliery	Deaths	Time of explosion	Seams affected
Sept. 8	Seaham	164	2.20 a.m.	Maudlin and Hutton
1882				
Feb. 16	Trimdon Grange	74	2.30 p.m.	Harvey
April 18	Tudhoe	37	1.15 a.m.	Brockwell
April 19	West Stanley	13	1.0 a.m.	Basty
April 25	Whitehaven	4	11.15 p.m.	Main Band
1885				
March 2	Usworth	42	8.58 p.m.	Maudlin

All the explosions with the exception of that at Whitehaven were in the county of Durham. It would be quite impossible in the space at our disposal to follow the successive steps in the minute analysis to which the authors have subjected each of these explosions. We should require, moreover, many of the numerous plans of the colliery workings with which the book is illustrated were we to attempt such a task. All that can now be done is to point out the characteristic features of the several explosions, and to indicate the general conclusions which the authors draw from the consideration of the various circumstances connected with them. We are conscious that in some respects this method of treating their work hardly does justice to the authors. It fails to convey any idea of the thoroughly scientific manner in which the Messrs. Atkinson's investigations have been conducted; of the minute and painstaking mode of their observation; or of the care and skill with which their deductions have been made. The authors, even in the earlier pages of the book, make their position in regard to the question of Gas *versus* Dust perfectly clear, but not even the most prejudiced opponent of the dust hypothesis can complain of the manner in which the evidence is presented.

The Durham explosions presented many features in common. In the first place no accumulations of gas were known to exist in quantity sufficient to cause the widespread destruction which happened, nor were such accumulations considered possible. In all these explosions the downcast shafts were more or less damaged. At Trimdon Grange, Tudhoe, West Stanley, and Usworth the explosions did not cross the downcast shafts; these were wet, and the roadways near them were damp. At Seaham the shaft was dry, and the explosion crossed it and extended far beyond it. In all cases the violence and flame of the explosions were confined to roads on which there was much coal-dust. The explosions were most violent in the intake and haulage roads, or between the downcast shafts and lamp-stations, *i.e.* in places where practically no gas was to be expected, and where naked lights were in constant use. The path of the explosion was in all cases that of the fresh air traversing the pit: in no case did it extend by means of the return air-way The return air-ways carry off the gases evolved in the pit, but are practically free from dust. In certain of the intake air-ways at Seaham and Usworth no coals were led, and they were consequently comparatively free from coal-dust: no traces of the explosions were observed in these roads. The explosions were in many cases arrested where the haulage roads were wet. In no instance did the explosion ascend or descend vertically through staples or shafts communicating with other planes of workings. If the explosions were due to gas, their extension would not be influenced by the direction of a communicating passage; on the other hand, very little coal-dust collects in vertical passages. In almost every case of an explosion which could with certainty be attributed to fire-damp, there is evidence that men have been alarmed and have attempted to escape from the workings before the actual occurrence of the disaster: in all the five Durham explosions there was no indication that any movements had taken place amongst the men suggestive of alarm; their bodies were found in the places where their work required them to be, close to their tools and lamps.

At Seaham, Tudhoe, West Stanley, and Usworth the explosions were simultaneous with the firing of shots in stone; in these cases the explosions occurred when the pits were occupied by stonemen and repairers and at the only time when the operations of the mines allowed the firing of shots. At Seaham, Tudhoe, and Usworth the shots were fired on a main intake air-road and at points where currents of air of between 20,000 and 30,000 cubic feet per minute were passing. At West Stanley the shot was fired, in stone, at a working place by a naked flame, and the air in the vicinity would probably contain a small quantity of fire-damp, but not sufficient in amount to show its presence in the safety-lamp or by itself to be explosive. In the other cases it is almost impossible to conceive that the air could contain any sensible quantity of gas. At Seaham it would be necessary to assume that the gas came down the shaft, or that there were three separate and simultaneous outbursts of it on the three main roads diverging from the shaft. At Tudhoe, where the air came direct from the surface by two shafts, it would be necessary to assume two separate and simultaneous outbursts. At Usworth the air had passed no working place, and could hardly have contained even a trace of fire-damp. At West Stanley no appreciable quantity of gas could be present in the main intakes, although a small quantity might be contained in the air near the place where the shot was fired.

There remains the Trimdon Grange explosion, which, was unconnected with shot-firing. There was distinct evidence that it originated with the ignition of gas at the light of a boy engaged at a pump in connection with some drowned workings from which gas was found to issue and that it extended with great force to parts of the pit more than a mile distant from its origin along the main intake air-ways.

Now all the circumstances connected with the Durham disasters make it almost certain that the main agent in the propagation of the explosion was dust, and in three out of the five cases it was dust alone. In four out of the five cases the immediate cause was shot-firing, *but in no instance was the shot blown out*. It is not at all necessary that the shot should be blown out to cause the ignition of the dust-cloud which the concussion raises in a dusty road. Properly fired shots show flame even when they dislodge the stone or coal; and the flame is often considerable if there has been an overcharge of powder, or if small coal or earth mixed with coal-dust has been used, as frequently happens, in the tamping. At Seaham, Tudhoe, West Stanley, and Usworth the flame of the shot ignited the dry inflammable dust dislodged from the roof or raised from the floor by the concussion of air which followed, and the explosion was propagated by fresh dust-clouds raised in the manner described by the Royal Commissioners. At Trimdon Grange an explosion of fire-damp operated in the same way: the violent movement of air resulting from the ignition of fire-damp and air raised a cloud of coal-dust into which the flame from the fire-damp passed, and the ignition of the coal-dust propagated itself as in the other cases, and, as in these, continued so long as it was fed by fresh fuel. This rapid ignition of dust containing upwards of 80 per cent. of carbon would result in the formation of large quantities of carbonic acid, and possibly even of the more poisonous carbonic oxide: when it is considered that it is impossible to live in air containing even 3½ per cent. of carbonic acid, the deadly character of the after-damp so formed is readily conceivable.

In striking contrast to the Durham explosions was that at Whitehaven. This was in a wet pit; the coal being worked was wet, and all the surroundings were damp, and free from dust. The cause of the explosion was gas, which was known to be in the pit, and frequently present in large quantities. Although it is probable that some 30,000 cubic feet of an inflammable mixture of air and fire-damp were ignited, the explosion was confined to a limited area of the workings, which extend to nearly three miles from the shafts. Seven men were within the district of the explosion, of whom three escaped. The survivors stated that all the men were alarmed by the appearance of gas immediately before the explosion, and hurried away. In the act of retreating the gas ignited at a lamp which was afterwards proved to have been defective and to allow of the passage of the flame. This the authors say was the most considerable explosion of fire-damp and air that they are acquainted with. They have personally investigated during the last twelve years almost all the explosions occurring in the North of England, and they cannot point to a case where there was direct evidence of so large a quantity of fire-damp and air exploding.

The moral of all this is obvious. It can scarcely be gainsaid that some of the most disastrous explosions of the last thirty years are primarily to be attributed to the practice of firing gunpowder in dusty mines. That under certain circumstances gunpowder can be used with safety is allowed. But the Royal Commissioners have issued a warning in no uncertain terms. They have convinced themselves that the abolition of the use of powder in dry and dusty mines will not generally involve any formidable inconvenience, inasmuch as the work which is accomplished by its employment both in coal and in stone can now be performed with equal efficiency, and at very little if any greater outlay, by other means. Unless, therefore, mining engineers, or those immediately responsible for the working of collieries, can devise some satisfactory method of minimising the danger due to dust, they will be compelled before very long, in deference to public opinion, to renounce the practice of blasting by means of gunpowder, or by any other agent which causes a flame.

T. E. THORPE

McLENNAN'S "STUDIES IN ANCIENT HISTORY"

Studies in Ancient History: comprising a Reprint of "Primitive Marriage." By the late John Ferguson McLennan. A New Edition. (London: Macmillan and Co, 1886.)

THE first edition of "Primitive Marriage" appeared in 1865, and the book was already extremely rare when, in 1876, it was reprinted as the first part of the "Studies in Ancient History." The reprint also soon became scarce, and while the influence of the author has been steadily growing, and almost all students of early society have come to attach great importance to his speculations, his principal writings have for some years been almost inaccessible. This new edition therefore supplies a real want, and it is doubly welcome for the sparing, but judicious, notes and appendixes which the editor, Mr. D. McLennan, has attached to his brother's book. "Primitive Marriage" broke ground in a new field of research, and, as the point of view was wholly novel, the collection, sifting, and marshalling of the evidence on which the argument was based was entirely pioneer's work. At the close of his life, McLennan was in possession of a much larger material; he had pursued his argument in new directions and to further conclusions, and on one or two points he had come to change his views. But new research had only confirmed the main lines of the argument sketched with so firm a hand in his original essay; and read with the *caveats* which his brother has introduced at one or two points—chiefly as regards the interpretation of the Levirate, and the prevalence of Agnation—the present reprint may be taken as generally representing, so far as it goes, the author's final conclusions on the subjects discussed. I say *so far as it goes*, for in many directions his conclusions had been added to and his views developed. The editor promises us a second volume, to consist for the most part of writings hitherto unpublished, which will throw a good deal of light on these new developments; meanwhile he has restricted himself in the notes "to certain matters on which the author had announced a change of view, and to certain others where circumstances had made an additional statement imperative." Of the additional statements, the most important is contained in two long notes appended to the essay on Morgan's "classificatory system" of relationships, in which it is clearly made out that Morgan's theory rests on misconception of the facts, and that the supposed classificatory system of relationship is not a system of relationship at all, but a system of terms of ceremonial or friendly address, used in conversation even between persons who are not related to one another in any way. This comes out so clearly in the cases about which we are best informed, that it is very questionable whether the facts so laboriously collected by Mr. Morgan can be used to throw light on the early history of the family.

From his plan of reprinting the book as it stood, with no more annotation than was absolutely necessary, the editor has departed only in one point. The appendix containing "additional examples of the form of capture" has been re-cast and enlarged upon the basis of a paper of J. F. McLennan published in the *Argosy* in 1866, but with additions from other and more accurate sources. The reasons for adopting this course are obvious: the new matter in this appendix could not conveniently have been reserved for the promised second volume, and the facts are so arranged and explained as to confirm the author's argument, and effectually dispose of the notion that the form of capture in marriage is to be explained by maidenly bashfulness.

It will be seen from this brief account that, sparing as the editor's additions are, they make the new edition of the "Studies" well worthy of the attention of those who already possess the book in its older form. And to the not small class of students of early society who know McLennan's work only at second hand or by one hasty perusal, it may not be unprofitable to say that this is emphatically a book of which a general knowledge is not sufficient, inasmuch as some of the most important and interesting points are precisely those which are almost sure to be missed on a first reading. For this, perhaps, McLennan himself is partly responsible, for in giving to "Primitive Marriage" the subordinate title "an inquiry into the origin of the form of capture in marriage ceremonies," he seems to fix attention on what is only the starting-point of a far-reaching research. In print and in conversation one often meets with the notion that the doctrines of marriage by capture and kinship through women only are mere archæological *curiosa*, and that for the study of later law and custom it is quite indifferent whether these things are true, or whether, on the contrary, mankind started from the first with male kinship. But the importance of McLennan's researches lies largely in the demonstration that the structure of society under a system of kinship in the male line which has been preceded by kinship through women cannot be the same as would be reached by a race which has had male kinship from the first. Other writers have taught a doctrine of the priority of kinship through women, but no one except McLennan has accurately developed the consequences of the doctrine, and shown how it solves a problem which, though ignored by most writers, is of the highest importance, namely, the origin of *gentes* within a nation. Like all really original thinkers, McLennan has for one of his chief merits that he recognised the existence of difficult problems in matters which ordinary people pass over without seeing any difficulty at all. And therefore precisely those passages in his writings which on a hasty reading seem needlessly laboured and proper to be skipped are found upon re-perusal to be particularly useful and stimulating.

A word may be said in conclusion on what is promised for the second volume. It is satisfactory to know (p. 75) that it will include a short essay on the origin of exogamy. And from a note at p. 176 it may be inferred that in this essay the origin of exogamy will be sought in a state of society where marriage by capture was an established custom. We are also promised (p. 63) an essay on the marriage law of the Australian Kamiraloi, one of those highly complex problems in which McLennan's powers of analysis ought to appear at their best. From notes on pp. 109 and 228 it appears that part at least of McLennan's hitherto uncollected essays in the *Fortnightly Review*, including the papers on Totemism, or "On the Worship of Plants and Animals" (1869-70), will also be republished. It is to be hoped that in these reprints the editor will allow himself, in one direction, greater freedom of annotation than in the present volume. The Totem papers are in some respects the least finished of McLennan's writings, the evidence of totemism in the nations of ancient civilisation being much too largely drawn from second-hand sources. This gives an appearance of weakness to the whole structure of the argument, which has been very prejudicial to the influence of a most original and striking investigation. In point of fact a few of the detailed pieces of evidence ought to be abandoned altogether, but enough remains to leave the substance of the argument unaffected, and this ought to be clearly brought out by notes, referring to original authorities of unquestioned reputation, or giving up statements that cannot be authenticated. Even in the present volume one misses some notes of this kind. The polyandria of the Athenians (p. 235) rests on better evidence than the story which Augustine cites from Varro (Clearchus *ap.* Athen. xiii. p. 556 d.). Again, the note at p. 47, in which an attempt is made to prove the existence of the form of capture among the Hebrews from the phrase "to take a wife," ought rather to have been withdrawn than again built upon by the editor at p. 181; and what is said of the marriages of the Persians at p. 219 *sq.* requires careful revision.

W. ROBERTSON SMITH

BRITISH HYMENOMYCETES

British Fungi, Hymenomycetes. By Rev. John Stevenson. With Illustrations. Vol. II. Cortinarius—Dacrymyces. Pp. 336. 8vo. (Edinburgh: William Blackwood and Sons, 1886.)

WE are glad to welcome this second volume so speedily after the first, although we fear that expedition has been secured by some sacrifice of efficiency. It is a misfortune when the reader is impressed at once with the feeling that a volume has been hurried out to meet certain exigencies. That feeling is by no means absent in scanning these pages. As soon as p. 165 is reached, and there is no longer Fries's "Monographia" to fall back upon, *descriptions* give place to *diagnoses*, notwithstanding the remarks in the preface, which would seem to regard diagnoses with something of contempt. From p. 166 to the end the *student* must be content with the diagnoses from Fries's "Hymenomycetes Europæi," although there might have been collected together valuable notes from Fries's "Systema," Observationes," and "Elenchus." Nevertheless some advantage has been taken of the few descriptions published in the letterpress to Fries's "Icones."

It is of considerable importance to students that a work which professes to include all British species, up to date, should satisfy all reasonable expectations. The first volume omitted some forty species, and the present is by no means perfect. We open at p. 232, and find under the genus *Solenia* one solitary British species recorded, that of *Solenia ochracea*. Surely our author could not have been ignorant of the fact that *Solenia anomala*, P., is still more common, and was recorded by Berkeley in the "English Flora" (p. 199) fully fifty years ago. Neither could he have forgotten that another species was included in Cooke's "Hand-book" (p. 329) under the name of *S. candida*, since corrected to *S. fasciculata*. As these specimens were collected near Batheaston, by no other than Mr. C. E. Broome, and confirmed by the Rev. M. J. Berkeley, no doubt can be entertained of their being authentic. Furthermore, the name was corrected and the species figured by Berkeley and Broome in the *Annals of Natural History*, December 1870, No. 1301. The fourth species is *Solenia stipitata*, Fuckel, of which there are specimens in the Kew Herbarium. It cannot be conceded that a "Flora" satisfies all reasonable expectations when in one genus only one of four species is recorded.

Turning to an allied genus, that of *Cyphella*, we seek in vain for *C. Curreyi* or *C. albo-violascens* (which may be identical), *C. cyclas*, Cke. and Phil., *C. punctiformis*, Fries, *C. villosa*, Pers., all but one of which are well-known and widely-distributed species.

Whether the species under the genera *Stereum* and *Corticium* might have been arranged in a manner more in accordance with modern ideas, and far more useful to the student in their identification, may be left an open question. Those who are not facile in the use of the microscope may find it convenient to follow Fries, who paid little attention to microscopical characters, but surely in a large and difficult genus, such as *Corticium*, no assistance should be despised.

We observe, with some surprise, the genus *Microcera*, of Desmazières, included in a work devoted to British Hymenomycetes (p. 308) with the intimation "no British species." The fact is that *Microcera coccophila*, Desm., which is the type of the genus, has been found in Britain, and is recorded on p. 556 of Cooke's "Hand-book," and furthermore it is also true that it is not a Hymenomycete at all, but the conidia of one of the *Sphæriacei*, and is included as such in Saccardo's "Sylloge Fungorum" (vol. ii. p. 513). This singular double error might have been avoided had some mycologist been consulted who had not confined his attention exclusively to the Hymenomycetes.

The limits of species is another open question, and it is scarcely advisable to make too much of the insertion of what some may regard as doubtful species in a "Flora" wherein the author is not free to give reasons in their favour; nevertheless, we venture to hint that *Polyporus armeniacus*, Berk. (p. 215), is generally admitted to be only a resupinate condition of *P. amorphus*, Fries, and should not be continued as a distinct species. *P. Herbergii*, Rost (p. 195), is placed as an ally of *P. sulphureus* in the section "Caseosi," whereas *P. cuticularis* is found (at p. 202) in "Spongiosi." Unfortunately for this arrangement, the two species (*P. Herbergii* and *P. cuticularis*) are so closely allied that sometimes it is difficult to distinguish the one from the other, except by the difference in size of the pores, and hence some regard them as varieties of one species. At any rate, there is no good reason why such closely-allied forms should be separated by four-and-twenty intermediate species.

The mention of localities for species throughout the work is so vague, that some explanation should have been offered. When only one locality is given, the inference which would be drawn by the majority of readers would be that no other British locality was known at the time for that particular species. That this conclusion would be wrong is manifest from *Hydnum Weinmanni* (p. 242), which may be taken as an example. The locality cited is "Bristol," but Bristol is not the only, or the most important station for this species in Britain, because it occurs plentifully in the neighbourhood of Carlisle. If the intention was simply to indicate the locality where the species was first found in these islands, then again we fancy it is inaccurate, because, as we believe, it was first discovered by the late Rev. A. Bloxam, at Gopsall. The only solution we could suggest is that "Bristol" is the locality mentioned in Berkeley's "Outlines," and it was accepted as the only authentic record, without inquiry. Some species are stated to be "common," others "frequent," and others "rare," and when, in the absence of any one of these terms, a single locality is given, it is a fair inference that only one locality was known to our author, and that was the reason why it was given. Assuming this to be the case, we fancy that a very large number of these single localities could be challenged as not unique.

In addition to a "Glossary" of five pages, we are glad to find a good index of genera and species, but we search in vain for any clue to the contractions, in some cases only a single letter, employed in quoting authorities. Under nearly every species follows a line or two, sometimes five or six lines, of hieroglyphics, to which figures are appended. It may be all clear enough to the Rev. John Stevenson what is intended to be conveyed by "Quel. t. 11, f. 1," or "Viv. t. 27," or "C. Illust., pl. 276," but who these illustrious persons are, or what they have done, to be curtailed in such wise, is nowhere indicated. Surely the author must have determined upon giving a key to these mysteries when he first commenced to employ them, and, in the hurry to issue the second volume, quite forgot the "students," even if he remembered the "scholars," and closed the book before he had finished his work.

A summary of the contents of these volumes, as they stand, exhibits the following results as compared with the last preceding work on the same subject:—

"Hand-book of British Fungi" ...	1044
Stevenson's "British Fungi"	1675

or, an addition of 631 species of Hymenomycetes since the year 1871. The majority of the additions have been made in the Agaricini, which stand thus:—

"Hand-book of British Fungi" ...	699
Stevenson's "British Fungi"	1183

or, an addition of 484 species, leaving only 147 species to be distributed over the residue of the genera of Hymenomycetes. These results are at any rate a justification, if any were needed, for the publication of a new work, especially when the older one is entirely out of print.

There can be no doubt that all that portion of the work which contains translations from the "Monographia" of Fries will be exceedingly valuable to British mycologists, and this extends through the whole of the first volume and 165 pages into the second; the only regret being that the few remaining species, which have not as yet been recorded in these Isles, were not inserted in brackets, or published as an appendix, so that the whole of Fries's excellent work might have been in the hands of every mycologist in this country. Perhaps even now such an appendix might be published, and no doubt it would meet with a hearty welcome.

Despite of such strictures as we have been impelled to make, we venture to hope that the present edition will soon be exhausted, and that its author will be called upon to prepare a new and revised edition, with a key to all the mysteries of the old one. M. C. C.

THE OCEAN

Der Ozean. Von Otto Krümmel. (Leipzig und Prag: Freytag-Lempsky, 1886.)

THE great interest which oceanographical studies have aroused within the last few years is shown in a marked manner by the publications destined to popularise the notions acquired respecting this vast and important chapter of physical geography. Not long after the appearance of the "Lehrbuch der Ozeanographie" by Boguslawski, whose untimely death has interrupted the publication of the second volume, we have a new and small manual by Dr. Otto Krümmel, whose name is already known to oceanographers.

This little treatise is clearly written, and the most important general notions concerning the physical geography of the sea are well stated, and discussed with ability. The author has succeeded in expressing briefly the essential notions about the ocean, which have been recently acquired by the *Challenger* and other deep-sea expeditions.

The author describes, in the first place, the ocean's surface and its subdivisions ("Die Meeresflächen und ihre Gliederung"); discusses the relation of oceanic and terrestrial areas from the point of view of their respective size; indicates the distribution according to hemispheres; and points out the classification he has adopted into *oceans*, properly so called, with their general systems of ocean currents, and *secondary seas*, which are more or less cut off from the great oceans. The *secondary seas* are again subdivided into *interior*, or *inter-continental*, and *border seas*, situated on the outer edges of the continents. The volume of oceanic water is then estimated. In the second chapter the interesting questions connected with the deformation of the level and surface of the ocean, owing to the attraction of the continental masses, are examined The depths and contours of the ocean basins are next pointed out, and the work of the *Challenger* and other deep-sea expeditions, together with the apparatus employed, is described. The observations of the *Challenger* upon the nature and distribution of deep-sea deposits are summarised. The physical and chemical properties of sea-water are set forth in a special chapter—the salinity of the ocean, its distribution and origin; the gas contents; the transparency and colour, are, in turn, treated of. After having made known the principal phenomena regarding the temperature of the ocean and its distribution, Krümmel treats of the glacial phenomena of the Arctic and Antarctic Oceans, pointing out the limits of the floating ice and icebergs in each region, and the influence of these regions on the questions of general oceanic circulation. The last chapter is reserved for a consideration of the movements of water, such as currents, waves, and tides.

Such is the general order and method of this manual. There is no attempt to give any general notion of the life of the shores, deep sea, and surface of the ocean, or of any of the phenomena due to organisms. The author shows himself to be everywhere *au courant* with the most recent discoveries in his subject. It would appear, however, that he has not had an opportunity of consulting the "Narrative of the Cruise of the *Challenger*," published last year, or he would have embraced in his descriptions some additional interesting details and general views. The work is illustrated by many woodcuts and small charts, some of which are instructive, others conveying little information to the reader, but when the low price of the book (one shilling) is remembered it would be unfair to criticise closely these illustrations. Dr. Krümmel has attained the object he had in view—to popularise in a scientific manner our knowledge relative to the physical geography of the sea, a subject full of interesting questions for all cultured minds. J. M.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Connection between Chemical Constitution and Physiological Action

As regards Dr. Brunton's letter in last week's NATURE (p. 617), I would express myself as more than satisfied with the personal explanation, but Dr. Brunton has not noticed the most important point to which I wished to call attention, viz. that whatever may be the value of my experiments, as showing a connection between physiological action and chemical constitution, the researches of Crum Brown and Fraser have really no bearing on the subject, for the simple reason that they had no knowledge of the chemical constitution of the re-agents they employed. There is an old receipt for cooking a hare which commences "*First catch your hare*," and in attempting to show the influence of change in chemical constitution on physiological action, it is well first to get a constitution. In the last edition of Watts's "Organic Chemistry" (1886) it is stated, "All these bases (the alkaloids), like the amines, are derivatives of ammonia, but their molecular structure is for the most part unknown." Even as regards inorganic compounds, our knowledge of their chemical constitution is not the most definite, but I believe that the arrangement of the elements in isomorphous groups expresses most clearly the resemblance in the chemical constitution of their compounds.

After again reading carefully Dr. Brunton's paper, I must confess that I cannot find anything showing the connection between chemical constitution and physiological action, except, perhaps, in the case of the alcohols. Here we have a class of bodies in which the different members of the series have probably the same relation to each other as the elements in the same isomorphous group, and it is an interesting fact that not only do they resemble each other in their physiological action, but that their toxic action increases with their molecular weight, as I have shown to be the case with the inorganic elements, where, in each isomorphous group, the toxic action increases with the atomic weight of the elements.

In conclusion, I would reprint an extract from a paper published forty years ago:—"A moment's reflection on the problems to be solved will suffice to show that experiments conducted with this class (inorganic) of substances are more likely to furnish useful results than those made with bodies derived from the animal or vegetable kingdom, although, owing to the striking effects caused by some of these substances, physiologists have mostly directed their attention to them. By so doing, however, we are employing re-agents with the properties and composition of which we are imperfectly acquainted, to the neglect of those on the nature of which chemistry has already thrown much light, for not only are we better acquainted with the more purely chemical properties of inorganic compounds, but their relation to heat, electricity, and molecular polarity has been to a considerable extent made out." JAMES BLAKE

Disinfection by Heat

IN Dr. Parsons's Report on Disinfection by Heat (NATURE, vol. xxxiv. p. 583) occurs the statement: "It appears that there are no tables or formulæ in existence by which the degree of humidity of the air corresponding to a given difference between the wet and dry bulb thermometers at these high temperatures can be ascertained." There are both tables and formulæ; but the tables are the numerical values for the formulæ, and such tables are to be found in Balfour Stewart on "Heat," Dixon's "Treatise on Heat," Blanford's "Meteorologist's Vade-mecum," and numerous works on the steam-engine.

Let the degree of humidity be represented by h; vapour-tension at dew-point by x; wet-bulb temperature by t, its vapour-tension by f; dry-bulb temperature by T, its vapour-tension by F; barometric pressure by b. Then, the theory of the dew-point gives

$$h = \frac{x}{F};$$

and

$$x = f - \frac{0{\cdot}382\,(T - t)b}{1115 - 0{\cdot}7t};$$

hence

$$h = \frac{f}{F} - \frac{0{\cdot}382\,(T - t)b}{(1115 - 0{\cdot}7t)F}.$$

The tables give the vapour-tension, consequently if T, t, and b are known, h can be found. At these high temperatures the degree of humidity would probably not be required very accurately. If within 1 or 2 per cent. of accuracy would suffice, the second term may be omitted. This results from the fact that the higher the temperature of the air, the nearer is the dew-point to the wet-bulb temperature.

The quantity sought then is $h = \frac{f}{F}$. Given $T = 299$, 299, and 249; $t = 146$, 165, 190; and taking F and f in pounds from a table in "Lardner on the Steam-Engine":—

$$(1) \qquad h = \frac{3{\cdot}5}{63} = {\cdot}05;$$

$$(2) \qquad h = \frac{5{\cdot}5}{63} = {\cdot}08;$$

$$(3) \qquad h = \frac{9{\cdot}5}{29} = {\cdot}32.$$

Here saturation is represented by unity. This is at once a short and simple method of calculating the degree of humidity at these very high temperatures. If the barometrical pressure were observed, and the long second term worked out, the results would not be materially different, but would be something less.

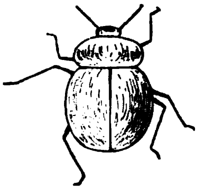
R. STRACHAN

11, Offord Road, London, N., October 25

The Beetle in Motion

MUCH has been written on "the horse in motion." Can any readers of NATURE supply me with references to published matter on the subject of hexapod progression?

The few observations I have made may be summed up in a few words. I use the letters r and l to signify the right and left legs respectively, and number the limbs from before backwards. When walking rapidly the appearance is as if l 1, r 2, and l 3 moved forward together simultaneously, alternating with r 1, l 2, and r 3. When the pace is slower it is seen that l 1 and r 2 start together and come down at about the same time, sometimes one sometimes the other being a little the first. Then, lifted almost but apparently not quite at the same time, l 3 starts. The motion of this leg being somewhat slower, and the limb having further to travel, the foot generally comes to the ground appreciably later than l 1 or r 2. The general effect is to produce, at the moments of pause between the strides, the position indicated in the figure, which differs considerably from the conventional position delineated by artists who seek to represent the beetle in motion.



C. LLOYD MORGAN

University College, Bristol

The Astronomical Theory of the Great Ice Age

IN Sir Robert Ball's paper on this subject, which appears in your last number (p. 607), that author states that the calculation given "has convinced him that Mr. Croll's theory affords an adequate explanation of the Ice age." It is more in the hope of obtaining from Sir Robert a statement of the grounds of this conviction than for the purpose of controversy that I write this letter.

It will of course be conceded that the frost and snow of a single winter, melted off during the following summer, would not produce an Ice age. But, on Sir Robert Ball's figures, the increase of winter cold at the period in question was accompanied by a corresponding and equal increase of summer heat. Why, then, should the latter prove insufficient to melt the winter accumulation of snow and ice in any locality where it now suffices to melt it?

The question is one of the joint result of two opposing forces. Both, under the supposed conditions, are intensified and equally intensified. How does this affect the result? More snow and ice is doubtless formed in the winter, but then more heat is employed in melting it during the ensuing summer. Why, then, was it not melted in any place where it is now melted? A kind of answer to this question may be extracted from the writings of Mr. Croll, but not, I think, a satisfactory one. I am therefore anxious (in common, I am sure, with many others of your readers) to hear the reply of Sir Robert Ball.

Llandudno, October 25 W. H. S. MONCK

The Enormous Loss from Ox-Warble

I VENTURE to solicit your co-operation in making some points better known in order that farmers may be better able to protect themselves from the enormous loss from warbles on cattle from the bot-fly, positive proof having been furnished that it largely exceeds 2,000,000*l*. to 3,000,000*l*. yearly! To begin: I appeal to those farmers who have somewhat studied the question to make it clear to those who have not done so that *each warble lump has a large maggot under it*, feeding on the juices of the hide or flesh. These lumps many call "health lumps" or "thriving bumps," and *seem to prefer that their cattle should have them*. It is readily seen how this serious fallacy has arisen, viz. from the fact that the warble lumps begin to show about Christmas (from the growth of the maggot under them), which also happens to be the time that the cattle receive their most nourishing food, and are then warmly housed or sheltered. But there could be no greater mistake than to think that the swellings from the ravages of these horrid maggots are proof of a thriving condition! A correspondent writes me: "Since reading recent issues on the ox-bot or warble-fly, I have visited several cattle markets and slaughter-houses to see for myself if the ravages of the maggots are so serious as the statements led one to believe. I must frankly state that what I have seen convinces me that the statements are much under the mark rather than over it. The first beast I handled showed 42 warbles, some only 3 to 6, whilst many others showed 30 to 70; and on examining hides at slaughter-houses this state of things was again confirmed (the warbles are more readily seen upon the *under*-side of the skin, and many are small ones that would not show as a lump. I am certain a farmer has only once to make such a visit to be not only convinced of the great loss, but also, if he has any neighbourly feeling about him, to make him call the attention of his brother-farmers to the subject."

I am anxious to indorse this recommendation, for the farmers should now satisfy themselves as to the actual state of the matter, as in a few weeks from now the warble lumps will have vanished, and I fear the farmers will hardly take protective measures during the summer, when the warbles are not visible, unless they are convinced; whilst seeing would be believing. I may remark that the following simple remedies are all efficacious to destroy the maggots: mercurial ointment and carbolised oil, to be applied with caution by a careful man; or, better still, quoting from the Report of the Royal Agricultural Society, "As a general application, safe in all hands, McDougall's preparation has proved excellently useful," and I have convinced myself it is the best and safest remedy that can be applied, not only for destroying the maggots, but, later on, as a wash to prevent the attacks of the flies. I would not have occupied so much of your space, but I am convinced this is a subject of national importance.
John Walker

Southport

P.S.—Farmers wishing for further information should read "Observations on Ox-Warble or Bot-Fly," 1884, and a second Report on "Ox-Warble or Bot-Fly," 1885, by Eleanor A. Ormerod, F.R.Met.Soc., &c. (London: Simpkin, Marshall, and Co.), and a new pamphlet called "The Bot-Fly," just issued by J. C. Jack, Grange Publishing Works, Edinburgh. This work fully defines every minute detail of the history, life, prevention, and losses sustained by the dreaded pest.

Aurora

The remarkable aurora borealis observed by Prof. Piazzi Smyth at Edinburgh on July 27 (Nature, vol. xxxiv. p. 312) seems to have been visible over a very great area. In my meteorological journal it is remarked on July 27 that the bright silver-clouds appeared beautiful between 9.30 and 11 p.m. "The colour of the northern sky above the silver-clouds was misty and brownish, though not cloudy." I had never seen such a tint in the sky. I have no hesitation in saying that the unusual darkness was the same as observed at Edinburgh. The fair white arc I did not see; clouds came up at midnight. It may be interesting to state that I also saw, on July 26 at 9.30 p.m., an aurora-like white cloud in the north-west. This cloud was very different from the well-known silver-clouds so often described in 1885 and 1886. On the 28th and 29th nothing extraordinary is mentioned in my journal, but on the 30th faint traces of the silver-clouds and again "a very strange yellow-brownish colour of the north and north-west sky" are remarked. The great aurora on March 30 we also observed very well at Königsberg.
F. Hahn,
Professor of Geography at the Königsberg University

Königsberg, Prussia, October 25

Earthquakes

It is always interesting to look for coincidences in the earthquakes in different parts of the world. In Nature, vol. xxxiv. p. 627, you announce that a violent earthquake was felt at Charleston and many other places in the United States of North America, on the 22nd inst. at 3 o'clock in the afternoon, *i.e.* 20h. 20m. Greenwich time. On the same day a very slight shock is recorded as having occurred at Neuchâtel, Switzerland, at 9h. 20m. evening, Berne time, *i.e.* 20h. 50m. Greenwich time. It is not impossible, but I must confess scarcely probable, that the faint shock at Neuchâtel was the re-percussion of the severe earthquake of North America.
F. A. Forel

Morges, Switzerland, October 31

In connection with Prof. O'Reilly's letters in Nature of October 14 and 28 (pp. 570, 618), and your notice of October 21 (p. 599), I supply a few data, which at first I thought of too little interest for your columns. At 6.12 p.m. local time (17h. 41m. universal time), on October 16, two shocks occurred with a short interval, the direction being approximately that of the meridian. The intensity was such as might be produced by very heavy carts passing.
H. du Bois

Strasburg, October 31

Meteor

This evening, at about 8.25, I saw a magnificent meteor, of a blue colour, falling a little to the left of the Pleiades.

Belfast, October 31 Joseph John Murphy

FREDERICK GUTHRIE

Frederick Guthrie was born in Bayswater on October 15, 1833. and was the youngest of six children. His father, Alexander Guthrie, was a tailor, carrying on business in New Bond Street, and is said to have been a man of literary taste and ability; that he was a man of cultivation is shown by the education he provided for his children, one of whom, Francis, early distinguished himself at University College, London, and at the London University, as a mathematician, and is now Principal of the South African College, Cape Town. As a boy, Frederick Guthrie was taught privately until his twelfth year by the late Henry Watts, F.R.S.; afterwards he was sent to University College School, then under the head-mastership of Prof. Key, whence he passed into University College, London. There he remained three years, the last two of which were devoted mainly to the study of chemistry, under Profs. Graham and Williamson, and of mathematics under De Morgan, a teacher with whom it was impossible for a young man of Guthrie's power to come into contact without receiving a life-long impress. There also he again came into contact with Watts, who was then principal assistant in Prof. Williamson's laboratory, and an intimate friendship was cemented with his old tutor that remained unbroken till the death of the latter. In the spring of 1854 Guthrie went to Germany to continue his chemical studies, and worked first at Heidelberg, under Bunsen, and then at Marburg, under Kolbe, where he took the degree of Doctor of Philosophy ("*summa cum laude*") in 1855, having previously graduated as Bachelor of Arts of the University of London. After returning to England he was appointed, in 1856, assistant to Dr. Frankland, then Professor of Chemistry in Owens College, Manchester. In 1859 he went to Edinburgh as assistant to the late Vice-President of the Council, who had just succeeded Dr. William Gregory as Professor of Chemistry in the Edinburgh University.

Two years later Guthrie accepted the Professorship of Chemistry and Physics in the Royal College, Mauritius. He arrived in the island in May 1861, and for six years he devoted himself to endeavouring to introduce and establish on a durable basis scientific instruction in the colony. Here one of his colleagues was Mr. Walter Besant, the eminent novelist, with whom he formed a friendship that remained intimate and uninterrupted through life. He returned to London on leave in 1867, and in 1869 he was elected Lecturer on Physics in the Royal School of Mines, a post which, with extended duties and modified title, he retained till his death.

In the spring and early summer of this year many of Guthrie's friends remarked upon his looking ill and seeming to be in low spirits. After a while he complained of a difficulty in swallowing, which presently became so serious that he was unable to take solid food. When at last he was prevailed upon to consult a physician, it was discovered that he was suffering from cancer of the throat. He sank rapidly during the last two or three months, and the inevitable end of his disease came on October 21. He was buried in Kensal Green Cemetery on the 26th.

Such were some of the chief outward and visible stages in Frederick Guthrie's career. Perhaps the first thing to strike any one on making his acquaintance was his strongly-marked individuality. His opinions were, much more than most men's, of his own forming, not simply picked up as they floated about in talk or in print. And his conduct followed his opinions: he did what he thought right, with very little regard to the consequences to himself, or to what might be thought of him by others. His scientific knowledge, too, was, much more than most men's, of his own getting, the result of his own observation and experiment. In others, also, he valued even a small scrap of self-gotten knowledge more than a large store of secondhand erudition. In this respect he sometimes went to excess, and, though not without mathematical knowledge, he was somewhat apt to underrate the scientific importance of the work of mathematical physicists in comparison with that of pure experimentalists. But even this mistake had root in the thoroughly sound conviction that it is the duty of a man of science to be a strictly faithful interpreter of the observed facts of Nature, and that, the further he ventures in the field of theoretical deduction the more room is there for self-deception. He seemed, however, sometimes to forget that phenomena do not present themselves to the natural philosopher ready clothed in words, and that all that can be expressed in human language is the conception formed in the mind of the observer. The true function of the mathematical physicist is in reality, as Kirchhoff has pointed out, nothing more than to find out the simplest statements that are consistent with observation.

Guthrie's devotion to science was complete and single-minded. He had a deep conviction of the value and dignity of any kind of genuine, self-forgetful, scientific work, and he knew how, if necessity arose, to claim the dignity due to a sharer in such work. But from affectation or vanity he seemed entirely free. His wonderful gift of humour and power of terse and telling speech made it easier for him, than for most men, to put down any approach to impertinence or presumption; but, except where he felt that a lesson was needed, he was most considerate of others, both in speech and action. He delighted in playful mystifications (see, for example, Prof. von Nudeln's letter in NATURE, vol. xxi. p. 185, on the "Potential Dimensions of Differentiated Energy"), but his drollery was never ill-natured. He was generous and kind-hearted in the extreme; as a friend he was steady and faithful. Although essentially a man of science, he had considerable literary attainments, and had an excellent knowledge of both German and French, while his powers of literary expression were remarkable. It will not astonish those who knew his ability in this direction to learn that as a young man he published (under the *nom-de-plume* of Frederick Cerny) a poem called "The Jew," and a metrical drama called "Logroño."

With regard to Guthrie's scientific position and achievements it may be remarked, in the first place, that he belonged to a class that was probably commoner in his generation, and in that which preceded it, than it is likely to be in the future—that, namely, of physicists who served their time as chemists. Until within the last twenty years or so the only accessible school of experimental science was a chemical laboratory, and consequently, for the last two generations, a large proportion of the most prominent physicists have been men who began their scientific career as chemists. Among many others, it may suffice to mention Faraday and Regnault. Guthrie's first published investigation seems to have been his dissertation on taking his Ph.D. degree; it was entitled "Ueber die chemische Constitution der ätherschwefelsauren Salze und über Amyloxydphosphorsaure." In the six years between taking his degree and going to Mauritius, he published eight or ten papers, mostly on points of organic chemistry—one of them, on the amyl group, contains the discovery of the therapeutic action of nitrite of amyl, and suggestions for its introduction into the pharmacopœia. His first physical investigations were published while he was in Mauritius, and included two researches into the formation of drops and one into the properties of bubbles. It is striking evidence of the reality of Guthrie's love of science and of his force of character that, under circumstances in almost all respects adverse to scientific work beyond what was required by his official position, he should have persevered steadily with his experiments and produced papers of great value. While in Mauritius he also published a paper on the iodide of iodammonium, and a pamphlet on "The Sugar-Cane and Cane-Sugar," and made complete analyses of the waters of the principal rivers of the island. After his return to England his scientific work was almost wholly confined to physics, but it is perhaps significant of the side from which he approached the study that the subjects that occupied him principally had relation to what is usually called in the text-books "molecular physics." Among many other researches the following may be specially mentioned: on the thermal conductivity of liquids; on approach caused by vibration; on stationary vibrations of liquids in rectangular and circular troughs; on salt-solutions and attached water (the results of this investigation were contained in a series of eight papers, and included the discovery of the substances named by Guthrie "cryohydrates," a class of solid hydrated salts which melt without change of composition, in most cases below 0° C.); on "Eutexia," an investigation into the properties, especially the melting-points, of metallic alloys and mixtures of salts.

As a teacher, it has been well said of Guthrie by one who knew him well, that "he did not desire merely to fill his pupils' heads, but to make them use them"—a far more valuable but more difficult result to attain. A large proportion of his pupils consisted of "certificated science teachers," and for these he introduced a system of instruction, consisting largely in making them construct with their own hands the apparatus required for their experiments, which was probably more fruitful (especially in the case of this particular class of pupils) than any other that he could have adopted.

In 1873 Guthrie issued to his scientific friends a characteristically worded little circular, which resulted in the formation, early in the following year, of the Physical Society of London, a Society which now includes, with very few exceptions, all the leading physicists of the United Kingdom. Through his intervention, permission was obtained from the Lords of the Committee of Council on Education for the meetings of the Society to be held in the Physical Laboratory of the Science Schools at South Kensington. He chose for himself the somewhat onerous post of "Demonstrator" to the Society, and in this capacity placed his time and the resources of his laboratory freely at the disposal of those who wished to exhibit experiments or apparatus at the Society's meetings. It was not till 1884 that he consented to become President.

In the early part of the present year he gave a course of three lectures before the Society of Arts on "Science Teaching," in which he advocated with equal vigour and humour the advantages of a training in experimental science.

Besides the poetical works already mentioned, and his numerous papers on scientific subjects, Guthrie was the author of the following books:—"Elements of Heat and Non-Metallic Chemistry," "Electricity and Magnetism," "Molecular Physics and Sound," and "The First Book of Knowledge."

He was elected a Fellow of the Royal Society of Edinburgh in 1859, and a Fellow of the Royal Society of London in 1873. G. C. F.

THE LONGEVITY OF GREAT MEN[1]

THE conclusion that the intellectual giants of the race are favoured by an abundance of years on the scene of their heroic activity, and are thus further differentiated from their more common fellow-men, seems natural, and has been accepted upon evidence which, in a less pleasing conclusion, would be considered ridiculously insufficient, and even false. The usual method of attempting to answer the question whether great men are longer-lived than others, is to prepare a list of the ages, at death, of a number of eminent men, take the average age, and compare it with a similar average of a number of ordinary men, or even with the average lifetime of the race, and in this way to make the results speak decidedly in favour of the superior longevity of great men. All that such a method can prove (and this it does prove) is that it takes long to become great. It neglects to consider that a select class of men is dealt with, and that, to be even potentially included in this class, one must have lived a certain number of years.

For example: in an article translated in the *Popular Science Monthly* for May 1884, it is argued that astronomers are a long-lived race because the average life-period of 1741 astronomers is 64 years and 3 months. An average human life is only 33 years; but as one cannot be an astronomer before adult life, the author takes the expectation of life at 18 years, which is 61 years, and thus makes an excess of over 3 years in favour of astronomers. He also divides his astronomers into four degrees of eminence, and finds that those of the first rank live longer than those of the second, and they in turn longer than those of the third, and so on, thus implying that the best astronomers are most favoured with years. The true conclusion is, that it takes longer to become a first-rank astronomer than it does to become a less eminent one.[2]

If great men were great from their infancy, and we had the means of ascertaining this fact, the method would be correct. But, as it is, we must define in some way or other what we mean by greatness, and then fix the average age at which it becomes possible to distinguish an amount of talent sufficient to enable its possessor to be enrolled in the ranks of the great as already defined. What is known as the "expectation of life" at any number of years tells the most probable age at death of one who has attained the years under consideration: a comparison of this age with the age at death of great men will decide whether they are longer-lived or not.

The attempt was made to select about 280 to 300 of the greatest men that ever lived.[3] Throwing out about 30 of the doubtful names, there remain 250 men, about whom the statement is hazarded that a list of the 250 greatest men, prepared by another set of persons, will not materially differ from our list, as far as all the purposes for which it is to be used are concerned. From this list I have selected at random a set of men of whom it was probably easy to fix the age at which they had done work which would entitle them to a place on this list, or work which almost inevitably led to such distinction: it is a date about midway between the first important work and the greatest work. The average of over 60 such ages is 37 years; which means, that, on the average, a man must be 37 years old in order to be a candidate for a place on this list. The real question, then, is, How does the longevity of this select class of 37-year-old men compare with that of more ordinary individuals? The answer is given by the expectation of life at 37 years, which is 29 years, making the average age at death 66 years. And this is precisely the age at death of these 60 great men; showing, that, as a class (for these 60 may be considered a fair sample), great men are not distinguished by their longevity from other men.

Further interesting conclusions can be drawn if we divide the men into classes, according to real psychological and physiological differences in the ways of manifestation of the several kinds of genius. It is almost surprising how well the ordinary trinity of faculties—intellect, emotions, and will—accomplishes this purpose. Greatness seems to appear either in a brilliant thought, a deep feeling, or a powerful will. Under men of thought would be included philosophers, scientists, historians, &c.; under men of feeling, poets, musicians, religionists, &c.; under men of action, rulers, commanders, statesmen, &c. Before comparing the relative longevity of these three classes of men, I assure myself that the period at which greatness begins to be possible does not materially differ[1] in the three classes, and, as was done in the former case, I exclude all cases of unnatural death. I find that men of thought live 69·5 years, or 3·5 years longer than ordinary men; while the lives of men of feeling are 3 years, those of men of action 5 years, shorter than those of average men,—a conclusion that agrees with the commonly accepted view on the subject. If we subdivide these three classes, we find, that, while all classes of men of thought live longer than ordinary men, the moralists live longest, scientists coming next; that among the men of feeling the religionists alone live the full period of life, while poets' lives are 5 years, and musicians' lives 8 years, too short; that, of men of action, rulers and commanders both fail to complete the full term of life by 4 years. One sees from these statements (which, however, in their detail at least, must be accepted with hesitation, owing to the fewness of examples) that the kind of psychical and physical activity pursued influences the life-period; that certain types of genius are apt to die young, while others are particularly favoured with a full allowance of years.

The question of longevity becomes important when we consider that through it the leaders of civilisation are allowed to exercise their important function a few years longer, thus enabling more great men to be alive at the same time; and that, by its tendency to be inherited by the offspring, the children of great men will begin life with a better chance of reaching maturity, and, in turn, of becoming important to the world, if, as we have reason to believe it would, the genius of their ancestors has left its traces in them. JOSEPH JASTROW

THE GEOLOGY OF THE LEBANON

WE are indebted to Dr. Carl Diener, of the University of Vienna, for an able monograph on the geological and physical formation of the Lebanon and surrounding districts, accompanied by maps, sections, and illustrations reproduced from photographs.[1] Notwithstanding the observations of Russegger, Fraas, and others, on the physical features and structure of this region, a complete monograph on its geology has long been a desideratum, and the work of Dr. C. Diener forms a fitting continuation of the survey of Lartet in Palestine, and of the Palestine Exploration Society in Arabia Petræa and the Jordan Valley.

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[1] From *Science*.

[2] Mr. Galton ("Hereditary Genius," p. 34) has allowed himself to neglect a similar consideration. In giving the number of men in each class that the population of the United Kingdom would have between certain ages, he gives 35 as the number of men of class *G* (a very high degree of eminence) between the ages 20 and 30, and only 21 such men between 40 and 50 years. But this cannot be true, because only a very small proportion of men could possibly attain the eminence requisite to be classed among the *G*'s in 20 to 30 years, while almost all (of those who will attain it at all) will have attained it before the end of their fiftieth year. And this consideration far outbalances the excess in absolute number of men between the former ages over those between the latter. Similarly the falling-off in the number of men of class *g*, *i.e.* idiots, from decade to decade, would be more rapid than in ordinary men,—a fact which the tables fail to show.

[3] The names were selected by three others and myself, while engaged in a study of what might be called the natural history of great men. The process of selection was most rigid and careful, by a system which it would take too long to describe.

[1] Mr. Sully (*Nineteenth Century*, June 1886) has shown that men of feeling are more precocious than men of thought; but the difference in the age at which their first great work is done, though in favour of men of feeling, is very slight indeed.

Down to a comparatively recent period, the ranges of the Lebanon and Anti-Lebanon were supposed to be formed of Jurassic limestones, but the observations of Oscar Fraas showed that this was an error, and that they are mainly formed of Cretaceous and Eocene limestones. It is only within the limits of a narrow belt at the western base of Mount Hermon that Jurassic beds really occur; this being their first appearance on proceeding northwards from Arabia Petræa. The formations overlying the Jurassic strata are referable to the "Neocomian" (?), Cenomanian, Turonian, Senonian, Eocene, and newer Tertiary periods; while great sheets of basaltic lava of late Tertiary age occur both to the north and to the south of the region embraced by the memoir.

Dr. Diener has worked out with great success the numerous lines of faulting and flexuring which the strata have undergone since their deposition, and which have been produced mainly during the Miocene epoch. Mount Hermon itself owes its position in a great degree to the elevation of its mass along the line of a great fault which coincides with its western base. Its beds of limestone, belonging to the age of the Lower Chalk of Europe, are disposed in the form of a low arch, the axis of which passes under the summit, and ranges in a north-north-east direction along the line of the heights of Anti-Lebanon. Other faults range along the southern and eastern flanks of the great dome-shaped mount which has thus been bodily upheaved in respect of the bordering strata. There can be no question that the system of terrestrial disturbances along which the Syrian mountains have been fractured and dislocated is the same as that which has given origin to the Jordan-Arabah depression; and amongst the lines of displacement traced out by Dr. Diener, we can have no difficulty in recognising that which is the actual prolongation of the leading fault of the Jordan Valley. This great line of fracture and displacement appears to enter the valley of the Leontes (Litany) at the western base of Hermon, where a complete change of the stratification takes place on either side, and the "Lebanon Limestone," with the subordinate Lower Cretaceous beds, are thrown into a nearly vertical position, and brought into contact with horizontal strata of the Upper Chalk (Senonkreide). It may therefore be inferred that the great valley of Cœle-Syria (El Bekâ'a), separating the range of the Lebanon from that of Anti-Lebanon, owes its origin, in the first instance, to the same system of faults which has caused the depression of the Jordan Valley, the original features having been modified by extensive denudation; and if we suppose that the primary line of fault reaches as far north as the Lake of Homs, in the valley of the Orontes, and as far south as the Gulf of Akabah, the distance through which this great line of fracture of the earth's crust will have been traced will amount to about 350 English miles.

Dr. Diener expresses some doubts regarding the former existence of glaciers in the Lebanon, notwithstanding the opinions of such observers as Hooker, Fraas, Girard, and others. Hooker especially identifies the mound upon which the grove of ancient cedars is planted as an ancient moraine. The author throws some doubt upon this view, because he was unable, after three hours of search, to find scratched or striated boulders, although he admits that, viewed in certain directions, the mounds do present the appearance of a terminal moraine. In reference to this subject, it may be observed that the position and altitude of the Lebanon Range makes it extremely probable that perennial snow, giving origin to glaciers, occupied the higher regions during the Glacial epoch. Amongst the Caucasus, which are only a few degrees further north, though somewhat higher, glaciers occur at the present day, and during the Glacial epoch the valleys were brimful of ice. Hence it would be strange if in the Lebanon it were proved that they had been entirely absent. The scarcity or absence of glacial striations, on which Dr. Diener founds his objection, is easily accounted for when we recollect that the blocks and stones consist of rather friable limestone which has been exposed through thousands of years to the effects of frost, heat, and rain. It is only when the surface of a rock, or of a boulder, has been protected by a coat of stiff glacial clay, that we can expect the striæ and scars to be preserved throughout a long period of time.

On another point Dr. Diener expresses his dissent from the views of previous observers, arising, as it seems to the writer, from his want of appreciation of the full effect of eroding agencies. The neck of land which connects the Râs Beyrût with the outer ridges of the Lebanon is formed of beds of stratified gravel or conglomerate rising from 120 to 150 feet above the sea. This is to all appearance an old sea-bed formed at a time when the land was submerged to the extent above indicated, during which Râs Beyrût was an island. The author cannot accept this view, because his observations of the coast-line of Syria, bearing on the present state of the harbours, do not appear to show a change of level of more than a few feet; less, in fact, than would be necessary to submerge the neck of land. On the other hand, he accepts the evidence offered by Lartet and the writer of a submergence of the coast of Southern Palestine and Philistia to an extent even greater than this, namely 200 feet and upwards; and he points to the evidence of great changes of level on the coast of Northern Syria and Asia Minor. May not the absence of raised beaches on the coast of Southern and Middle Syria be due to the waste caused by the wave action of the Mediterranean, which would tend to carry away such soft materials during the period of emergence where exposed and unprotected? In another case the author throws doubt on the observations of Dr. Post regarding the presence of shell-beds at levels of 150 to 250 feet near Lâdikîeh, an account of which appeared in Nature, vol. xxx. p. 385, and which is given with much detail. It seems an instance of hypercriticism to call in question an authenticated statement merely on the ground that the author was unable to personally verify it.

The above instances will, however, go to show with what care and labour Dr. Diener has accomplished his task, and he is to be congratulated upon the production of a work which will doubtless be considered a standard of reference regarding the physical history of the Syrian mountains. I may perhaps be allowed to remark that his admirable geological map would have been improved by following the English custom of showing the dip of the strata by means of small arrows, and of distinguishing between ordinary boundaries of formations and those which are produced by faults and fractures, and the book itself would have been rendered easier for reference by an index.

Edward Hull

AUTUMNAL FLOWERING

THE "extraordinary gooseberry" season seems to have set in this year with more than usual severity. Country clergymen and amateur gardeners, who would see nothing unusual in the autumnal flowering of a hybrid perpetual rose (which reminds them, perhaps, of their old school-days, when they read of "*biferique*

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[1] "Libanon; Grundlinien der physischen Geographie und Geologie von Mittel-Syrien." (Wien, 1886.)

rosaria Pæsti"), are moved with astonishment at the sight of a second crop of flowers on an apple-tree or a laburnum. Common as the phenomenon is, however, not many persons, even among botanists, bestow a thought as to how it is brought about. Gardeners recognise two distinct modes in which flowers may be produced, either from the "old wood," meaning the wood formed in the previous season, or from the shoot of the present year's growth. A rhododendron with its flowers packed up in a "winter-bud" destined to unfold in spring, an apple or a laburnum with their winter-buds at the ends of short contracted shoots or "spurs," afford illustrations of the one type, while a rose, with its newly-formed shoots crowned with one or more rose-buds, supplies an example of the latter. There is the same sort of difference between these two kinds of flowers that there is between the so-called "annual" plants whose course of life is outrun in a single season, and "herbaceous perennials" which die down in winter, leaving a winter-bud to carry on the work when circumstances become propitious in spring. The second growth of flowers in autumn may, therefore, be due to two different causes. In the one case it is an anticipation of spring; the flowers being produced afore time. Conditions of growth being persistently favourable, the winter-bud, instead of remaining dormant, bursts prematurely into growth, and repeats in autumn what its predecessor had done in spring. The great difficulty in such a case is to explain why one bud, or at any rate only a small proportion of the total number of buds, acts in this way when the circumstances of the case would appear to be substantially alike in all. To talk of the individuality of buds is to denote a fact which every observer must be conversant with, but which does not supply any explanation. In the second class of cases the flowers are, as in "hybrid perpetual" roses, placed at the ends of some of the shoots of the year. In this case gardeners have availed themselves of what was originally an occasional tendency to continue the development of flowers on the end of certain shoots, and have, as it were, converted an accidental into a constant occurrence. Doubtless they might do the same in the case of the laburnum, were they so disposed. It is here that the skill of the gardener comes in, and even enables him, to some extent, to baffle adverse climatic influence and induce a plant, as a regular thing, to flower twice in a season, or even more or less continuously, when, if left to itself it would either not do so at all, or only in a fitful, uncertain manner. It is worth notice, too, that these second blooms are often (but by no means invariably) malformed. Some rhododendrons now before me are so, while the double-flowered apples that one occasionally sees are always, in my experience, formed on the midsummer shoots of the tree. So, again, with pears, the second crop of flowers is usually produced on shoots of the year, and very generally the flowers are more or less imperfect or misshapen. The "Napoleon" pear behaves in this way every year. Every year, too, I am indebted to Mr. Burbidge, of the Trinity College Botanic Garden, Dublin, for specimens of "Bishop's Thumb" pears, produced on the summer shoots. These pears are more like fingers than thumbs, and are destitute of core. The flower-stalk swells up as usual, and produces an eatable pear, but the carpels and seeds are conspicuous by their absence. The developing force has been energetic enough to produce flower- and fruit-stalk, but it has failed in the more essential process of seed- and embryo-formation. Possibly in some cases the absence of seed may be the result of want of fertilisation. It may be that in the flowers some at least of the carpels are present with their contained ovules, but, owing to the want of effective fertilisation, they have dwindled away and left no trace.

It would be a curious and important matter to ascertain whether, and to what extent, this repeated flowering process exhausts the plant. If no seed were produced the extra outlay of energy would probably not be severely felt. But every rose-grower knows how great are his losses, and how difficult it is to keep his "standards" in good form and good health. Of course there are many causes for this, but it is not unreasonable to suppose that one of them arises from exhaustion from continuous flowering, which produces a condition that predisposes to disease.

Another phenomenon of a somewhat similar character is very commonly met with this autumn, although, not unnaturally, it does not attract so much attention. I allude to the production of buds and leaf-shoots on the partially withered stems of herbaceous perennial plants, such as various species of Epilobium, Malva, &c. The branches of these plants usually dry up after flowering, leaving only a rosette of leaves or a winter-bud to carry on the growth next season; but occasionally they retain some amount of vitality, and, as at this season, produce a new generation of shoots from the old ones.

These variations show how artificial are the distinctions denoted by the terms annual, perennial, herbaceous, and the like, and they show what a wide range of physiological diversity may exist within the limits of the same species. MAXWELL T. MASTERS

ARROW-RELEASE[1]

At the commencement of this very interesting and instructive monograph, Prof. Morse tells us that when he began collecting data illustrating the various methods of releasing the arrow from the bow, as practised by different races, he was animated merely by curiosity; nor was it until he had accumulated quite a collection of sketches and other memoranda on the methods of arrow-release, not only of existing but of ancient races, as shown by frescoes and rock-sculptures, that he realised that even so trivial an art as that of releasing the arrow might possibly lead to interesting results in tracing the affinities of races. Hence he publishes in the present pamphlet the data which he has thus far collected, in the hope that further material may be secured for a more extended memoir on the subject. The great difference which Prof. Morse observed between the ordinary English and Japanese methods of using the bow first led him to investigate the subject, with the curious results to be presently narrated. The various forms of release, with their different modifications, are classified, and perhaps Prof. Morse's investigations may be most succinctly described by using his classification.

(1) *Ordinary Release.*—This is the simplest form of release, and is that which children all the world over naturally adopt in first using the bow. It consists in simply grasping the arrow between the end of the straightened thumb, and the first and second joints of the bent forefinger (Figs. 1 and 2). With a light or weak bow, says Prof. Morse, this release is the simplest and best; it makes little difference on which side of the bow the arrow rests, provided the bow is held vertically. On the other hand, however, a stiff bow cannot be drawn in this way, unless one possesses enormous strength in the fingers. This simple or primary release is that in use amongst the Ainos of Yezo, by the Demerara Indians, apparently also by the Utes. The Navajos employ it when shooting at prairie dogs, so that the arrow will not penetrate the ground if it misses its mark; so do the Chippewas. The Micmac Indians of the Cascapedia settlement, on the north shore of the Bay of Chaleur, used it, and it is said that the other tribes in this part of Canada draw the arrow in the same way. A member of the Penobscot tribe at Moosehead Lake, seemed incredulous when Prof.

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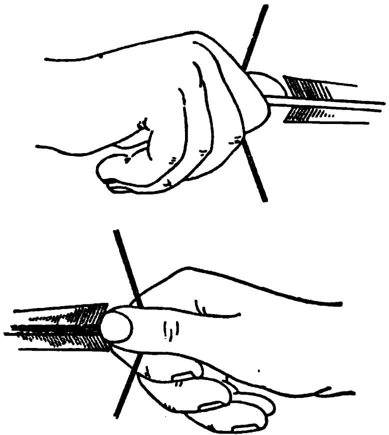
[1] "Ancient and Modern Methods of Arrow-Release." By Edward S. Morse, Director Peabody Academy of Science. Essex Institute *Bulletin*, October-December, 1885.

Morse told him that there were other methods of drawing the arrow.

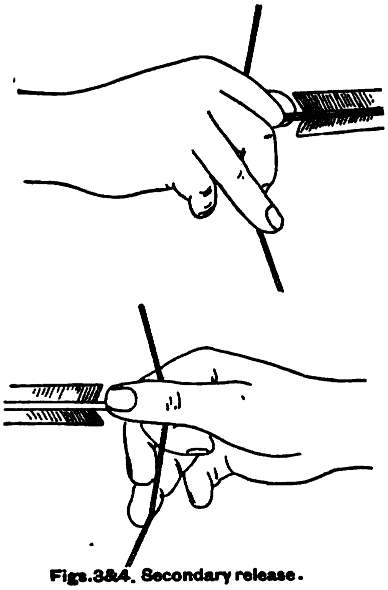
(2) *Secondary Release.*—This is a direct outgrowth from the primary release. It consists in grasping the arrow with the straightened thumb and bent forefinger, while the ends of the second and third fingers are brought to bear on the string to assist in drawing (Figs. 3 and 4).



Figs. 1&2. Primary release.

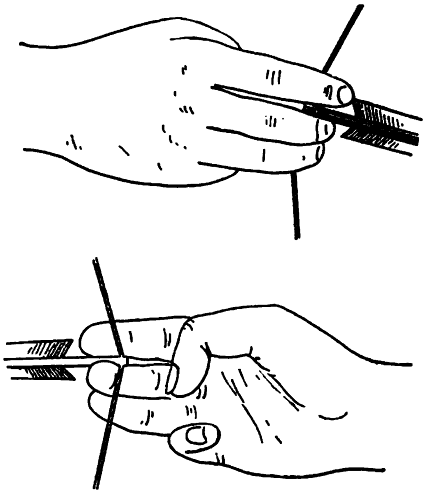
The Ottawas and Zuñi Indians practised this, as also did the Chippewas of Northern Wisconsin.

(3) The *Tertiary Release* differs little from the secondary. The forefinger, instead of being bent, is nearly straight, with its tip, as well as the tips of the second and third fingers, pressing or pulling on the string, the thumb, as in the primary and secondary release, active in assisting in pinching the arrow and pulling it back. This is used amongst various tribes of American Indians—Sioux, Araphoes, Cheyenne, Assinboins, Comanches, Crows, and Blackfeet. The Siamese, too, practise this release, with the difference that one finger only is used on the string instead of two. It appears, too, from Mr. Man's recent paper before the Anthropological Institute, that the Andaman Islanders use this method.

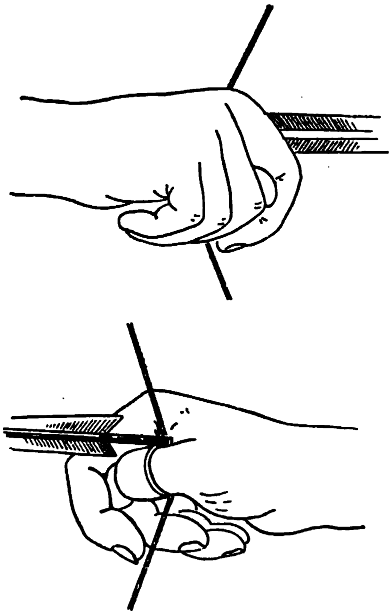


Figs. 3&4. Secondary release.

(4) *The Mediterranean Release.*—This release has been in vogue among the northern Mediterranean nations for centuries, and among those of the southern Mediterranean for tens of centuries, and is the oldest release of which we have any knowledge. It is practised to-day, continues Prof. Morse, by all modern English, French, and American archers, and is the release used by the European archers of the Middle Ages. It consists in drawing the string back with the tips of the first, second, and third fingers, the balls of the fingers clinging to the string, with the terminal joints of the fingers slightly flexed. The arrow is held lightly between the first and second fingers, the thumb straight and inactive (Figs. 5 and 6). A leather glove or leather finger-strings are worn, as Roger Ascham expresses it in his "Toxophilus," published in 1584, "to save a man's fingers from hurtinge, that he may be able to beare the sharpe stringe to the uttermoste of his strengthe." In this release, the arrow must be to the left of the bow vertical. The Eskimo of Alaska employ this release, using, however, only the first and second fingers in drawing the string, and it appears to be almost universal in the Arctic regions.



Figs. 5&6. Mediterranean release.



Figs. 7&8 Mongolian release.

These four releases may be considered, Prof. Morse thinks, as successive modifications of each other; but the next release is an entirely independent form, having no relation to the other.

(5) *The Mongolian Release.*—In this the string is drawn by the flexed thumb bent over the string, the end of the forefinger assisting in holding the thumb in position (Figs. 7 and 8). The arrow is held at the junction of the thumb and forefinger, the base of the finger pressing the arrow against the bow. For this reason the arrow is always placed to the right of the bow vertical. This release is characteristic of the Asiatic races, such as the Manchu, Chinese, Corean, Japanese, and Turk. The Persians also use it. The thumb is protected by a guard: the Manchus, Chinese, and others use a thick ring worn near the base of the thumb. It may be made of any hard material, such as horn, bone, ivory, quartz, agate, or jade. The Japanese archer uses a glove consisting of the thumb and two fingers.

These are the principal and most efficient forms of release, although doubtless there are others. Of the methods employed by ancient peoples, as represented in manuscripts, sculptures, &c., the Assyrians at one stage of their history appear to have used the primary form, while subsequently they used the secondary, and still later the Mediterranean release. The ancient Egyptians appear to have practised three, if not four, definite and distinct methods of release, but many of the representations in the old sculptures are evidently purely conventional, while some are clearly impossible. Following on these, Prof. Morse discusses the methods employed in ancient Greece, Persia, Japan, China, India, Mexico. Here he is naturally on less secure ground, for he has to endeavour to spell out a conclusion from various and conflicting positions of the hand in various ancient graphic representations of life amongst these peoples. The discussion involves a considerable amount of detail and numerous woodcuts by way of illustration, for which the reader must be referred to Prof. Morse's pamphlet. We must content ourselves with reproducing briefly his conclusions, which, it will be understood, are at present for the most part provisional, pending additional information and wider discussion. The persistence of a particular release in a people is well illustrated in the case of the Ainos. For centuries the Ainos have battled with the Japanese, and must have been mindful of the superior archery of their enemies; indeed, on all hands, with the exception possibly of the Kamchatdales to the north, the Ainos have been surrounded by races practising the Mongolian release, and yet have adhered to their primitive methods of shooting. The two strongest releases—both perhaps equally powerful—are the Mediterranean and Mongolian, and it is interesting to note that the two great divisions of the human family who can claim a history, and who have been dominant in the affairs of mankind, are the Mediterranean nations and the Mongolians. For several thousands of years each stock has had its peculiar arrow-release, and this has persisted through all the mutations of time to the present day. Language, manners, customs, religions, have in the course of centuries widely separated these two great divisions into nations. Side by side they have lived; devastating wars and wars of conquest have marked their contact; and yet the apparently trivial and simple act of releasing the arrow from the bow has remained unchanged. At the present moment the European and Asiatic archer, shooting now only for sport, practise each the release which characterised their remote ancestors. The following classified list shows in a general way that the primary, secondary, and tertiary releases are practised by savage races to-day, as well as by certain ancient civilised races, while the Mediterranean and Mongolian releases, though originating early in time, have always characterised the civilised and dominant races. The exceptions to this generalisation are curious: the Little Andaman Islanders practise the Mediterranean release, and those of the Great Andamans the Tertiary; various groups of Eskimo practise the Mediterranean release, and have designed a distinct form of arrow for this method.

Primary Release.—Savage: Ainos, Demerara Indians, various North American tribes; civilised: early Assyrian, Egyptian, and Grecian (?)

Secondary Release.—Savage: some North American tribes; civilised: later Assyrian and Indian (?)

Tertiary Release.—Savage: North American tribes, Great Andamans; civilised: Siamese, Egyptian, Grecian, and Mexican (?)

Mediterranean Release.—Savage: Eskimo, Little Andamans; civilised: European nations now, and the archers of the Middle Ages, later Assyrian, early Egyptian, Arabian, Indian, and Roman.

Mongolian Release. — Manchus, Chinese, Coreans, Japanese, Turks, Persians, Scythians, Egyptians (?)

In conclusion, Prof. Morse expresses a belief that the method of using the bow may form another point in establishing or disproving relationships, in identifying the affinities of past races. Travellers and explorers should not content themselves with observing the simple fact that such and such people use bows and arrows, but they should accurately record (1) the attitude of the shaft hand; (2) whether the bow is held horizontally or vertically; (3) whether the arrow is to the right or left of the bow vertical; and (4) whether the extra arrows are carried in the bow hand or shaft hand. The method of bracing the bow is of importance also. While anxious to get information respecting the arrow-releases of tribes and peoples, he is particularly desirous of hearing about those employed by the Veddahs of Ceylon, the hill-tribes of India, African tribes, and those of South America, especially the Fuegians. Such material, in the shape of descriptions, photographs, drawings, and if possible specimens of bows and arrows, may be sent to Prof. E. S. Morse, Peabody Academy of Science, Salem, Massachusetts, and will be acknowledged and used in a future publication on the subject.

CLIMATOLOGY OF THE CROYDON DISTRICT[1]

IN a little tract of thirty-six pages, which has just appeared in the *Transactions* of the Croydon Microscopical and Natural History Club, Mr. Eaton has discussed the climatology of this part of England with a skill, clearness, and fairness seldom met with in local climatologies. The observations of temperature, which were conducted on the same systematic plan with Stevenson's screens, were made at seven stations, these being, in the order of their heights, Park Hill, Addiscombe, South Norwood, West Norwood, Waddon, Wallington, and Beddington. The periods selected for discussion are the five years 1881 to 1885 inclusive. The stations are included within an area measuring 4 miles from north-east to south-west by 2½ miles from south-east to north-west. The monthly results are given on fourteen pages with satisfactory fullness; and with them are conjoined, for the sake of comparison, the corresponding records of temperature at the Greenwich and Kew Observatories.

The heights and mean temperatures of the five stations from which observations are available for the whole of the five years are these:—Beddington, 102 feet, 48°·8; Waddon, 156 feet, 49°·0; South Norwood, 190 feet, 49°·4; Addiscombe, 202 feet, 49°·3; and Park Hill, 259 feet, 49°·4,—Park Hill, the highest station, being thus 0°·6 warmer than Beddington, the lowest station. This subversion of the general rule that the temperature diminishes with greater elevation is shown to be due to the frequency with which, on clear calm nights, the air in contact with the ground is cooled and rendered denser by radiation, and thereafter descends to the low-lying grounds of the valleys, displacing the warmer air below. During the unusually dry clear months of January and July 1881 the mean temperature of Park Hill exceeded that of Beddington by 3°·5 and 2°·5 respectively. Hence the first three of the five stations which are on sloping ground have, though at greater elevations than the other two stations below, higher mean temperatures.

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[1] "Report on the Temperature and the Rainfall of the Croydon District, 1881-85," by Henry Storks Eaton.

This peculiarity in the distribution of the night and the winter temperature becomes the more intensified as the valley is deeper and its sides steeper, and as calms and light winds prevail. Thus at Klagenfurt, situated in one of the valleys of the Tyrol, the mean temperature of January is 20°·7, whereas at the station of Obergipfel, about seven miles distant and 4270 feet higher, the mean for the same month is 19°·9, being thus less than a degree lower than that of Klagenfurt. The subject is one that has seldom received the earnest attention it deserves, particularly in drawing the isothermals of the globe. The Croydon Club would make a clear addition to their observing-system if new stations were established on knolls in the valley of the Wandle for the further prosecution of this inquiry.

The means of temperature from Greenwich and Kew would have had real value in this inquiry if Mr. Eaton could have availed himself of observations made at these Observatories with thermometers exposed in the Stevenson screen. But, as pointed out, the different modes of exposing the thermometers render the results of the three systems of observing incomparable *inter se*. Thus the mean of the daily highest temperature of August for the five years is 72°·5 for Greenwich, and 69°·5 for Kew.

The rainfall has been far more extensively observed in the district, the returns of no fewer than seventy stations being available. Grouping the stations according to height, the annual amounts at stations below 200 feet show a mean of 23·27 inches; 200 to 400 feet, 25·39 inches; 400 to 600 feet, 29·12 inches; 600 to 800 feet, 31·66 inches; and above 800 feet, 31·36 inches. The largest amounts of rain occur not on the ridge of the North Downs, but some distance on the lee-side in regard to the prevalent rainy south-westerly winds; and the amount at like elevations seems also to diminish from west to east. As regards the monthly rainfall, the depth is greater in the upper groups; but the ratios of the monthly to the annual fall show that in spring, but more particularly in summer, there falls proportionally a larger amount of rain in the lower group of stations, whose average elevation is 193 feet. The relatively large increase in the summer rainfall over low-lying plains is one of the most striking facts in the geographical distribution of the rainfall, and is probably due to the physical causes concerned in the development of thunderstorms.

NOTES ON THE RECENT SWARMING OF APHIDES

THE immediate cause of the sudden appearance of clouds of insects in certain localities is not very apparent, but it may be surmised that the predominance or scarceness of their natural insect foes has much control over the phenomenon; added to which must be taken into account the effects of weather and temperature. A few days ago I had a notice from an obliging Birmingham correspondent, Mr. George Baker, who kindly furnished me with the following particulars:—

On October 5 the town of Mansfield, on the borders of Sherwood Forest, was visited by a cloud of Aphides, which swarmed in the town and over the country round, across an area of many miles. The town was visited "literally by millions; every one, as they walked along, waving their handkerchiefs or newspapers before their faces to avoid inhaling the insects. . . . Wet paint was covered by a mass of these black Aphides." This swarm continued with decreasing numbers throughout five days, and heavy rain during part of this time did not seem much to affect them. On the road to Nottingham these insects were noticed as engaged in singular gyrations and undulatory dances above the tops of the spruce-firs, there forming dense pyramidal columns.

A similar cloud, but less remarkable as to numbers, was observed about the same time at Birmingham; which, however, as the town must be at least 50 miles distant, can be scarcely considered as forming a part of this same swarm. Possibly similar causes operated to produce the like phenomenon in both places.

These insects proved on examination to be *Rhopalosiphum dianthi* of Schrank, which is identical with *Aphis persicæ* of Morren, and *A. rapæ* of Curtis, and *A. vastator* of Smee. It is a veritable pest in some years, doing considerable damage to turnip, mangel, and other crops, and in our gardens injuring our peach-trees. This present notice of its swarming is, however, by no means unprecedented.

In September and October 1834 Morren noted an immense swarm all over Belgium, and states his belief that it came across the sea from England. He says they obscured the light of day, and covered the walls of the houses so as partially to conceal them. Gilbert White notes that in August 1785 the people of Selborne were surprised by a swarm of "smother flies." Those that were walking in the street found themselves covered with these insects, which blackened the hedges and vegetables round. White thought these might be emigrations from the hop-gardens of Kent and Sussex, and from those near Farnham. If so, the species differs from the insects above noticed.

The choice of high objects to dance over is not confined to Aphides, *e.g.* many of the Tipulidæ. The singular persistent dance of *Anthomyia meteorica* over the heads of horses is familiar to all. G. B. BUCKTON

NOTES

A MOST attractive group of birds has just been placed by Prof. Flower in the great hall of the Natural History Museum at South Kensington. The case is intended to illustrate the hybridisation of species in a state of nature, and the species selected are the hooded and carrion crows (*Corvus cornix* and *C. corone*) and the European and Asiatic goldfinches (*Carduelis elegans* and *C. orientalis*). The series of these birds has been presented to the Museum by Mr. Henry Seebohm, who procured the specimens himself during his travels in Siberia. The case of the crows is one of the few instances known of actual wild hybridisation, though many more are suspected, especially among the game birds. It is certain, however, that wherever the colonies of hooded crows meet the carrion crow throughout the Palæarctic region the two species interbreed freely, and the result is shown in the young, the gray saddle-back of the hooded crow exhibiting a considerable admixture of black owing to the strain of *C. corone* in the parentage. The case of the goldfinches is not quite so completely proved, but is apparently a parallel instance of hybridisation. The British Museum has been for some time indebted to Mr. Seebohm for very valuable presents of birds, which have been mounted in the bird-galleries. Not long ago he gave a specimen of Ross's gull (*Rhodostethia rossi*), one of the rarest of the *Laridæ*, and a species which was a *desideratum* to the national collection. He presented also, last year, a fine case of Steller's sea-eagle (*Haliaëtus pelagicus*) from Kamchatka.

THE Geodetic Conference began its meetings in Berlin last week. The countries represented are Belgium, by two delegates; Denmark, by one; Germany, by fourteen, including Prof. Dr. Förster, of the Royal Observatory, Prof. Helmholtz, Dr. W. Siemens, and Colonel Golz, of the Trigonometrical Survey: France, by two, namely, MM. Faye and Tisserand; Italy, by one; the Netherlands, by one; Norway, by one; Austria, by three; Portugal, by one; Roumania, by two; Russia, by two, including Dr. von Struve, of the Observatory at Pulkowa; Sweden, Switzerland, and Spain, each by one. England, strange to say, is not represented; nor has any one come from the United States. Prof. Dr. Förster, of Berlin, was elected President, and Dr. von Struve, of Pulkowa, Vice-President of the Conference. In his opening address, Herr von Gossler, Prussian Minister of Public Worship, indulged in some general observations as to the progress and aims of geodetic science, and, in the name of the Prussian Government, thanked the various foreign deputies for their appearance in Berlin. The chief task of the present Conference has been to settle the organisation of the central geodetic bureau, which is to have its permanent seat in Berlin, in connection with the Geodetic Institute of Prussia, founded by the late Lieut.-General von Bäyer. It was at the instance of Lieut.-General Bäyer that the first constituent international meeting of geodetic experts was held in Berlin in 1864, and it is by the establishment of a central international bureau here, supported by quotas from the various countries which it represents, that it is intended to preserve to Prussia the leading part she has always taken in promoting the science of earth-measuring and all its kindred branches. The permanent Committee elected includes Prof. Hirsch, of the Neuchâtel Observatory (Secretary), Professors Förster (Prussia), Sande (Holland), Faye (France), Ferrero (Italy), Ibannez (Spain), Ragel (Saxony), Oppolzer (Austria), Stepnicki (Russia), and Zachariae (Denmark). The next Conference will be held in 1887 at Nice, on the invitation of M. Bischoffsheim, owner of the great Observatory there. Before separating, the Conference passed a resolution requesting the Prussian Government to invite other States to join the International Geodetic Society.

AT a recent meeting of the Common Council it was decided that it be referred to the Gresham Committee to consider whether the moneys now paid for lectures under the provisions of Sir Thomas Gresham's will might be devoted to the encouragement of students destined for commercial careers acquiring a useful knowledge of modern languages, with instructions to confer with the Mercers' Company, and to report thereon forthwith.

THE Professor of Physics of the University of Vienna, Dr. Victor Pierie, died suddenly of appoplexy in his laboratory on Friday last.

AT the Potato Centenary on December 2 and 3, to which we have already referred, the following subjects for conferences have been proposed:—First day, Morning: (1) historic consideration of the question, Whence came the potato to England? (2) the Incas and their cultivation of the potato; (3) distinct wild species of the potato as at present recognised; (4) the production of varieties by cultivation. Afternoon: (5) the potato disease; (*a*) historic sketch, (*b*) our present knowledge of the disease. Second day, Morning: (1) proposed methods for preventing the disease; (2) methods for using partly diseased potatoes; (3) methods for storing and preserving potatoes. Afternoon: conference of cultivators on rates for transport of potatoes.

THE French Government has granted the funds required for the completion of the Algiers Observatory, which will be in full operation next spring. Two assistant astronomers have already been sent to join M. Trépied, and two others will be selected from among the pupils of the School of Astronomy this winter. A special Congress will be held in Paris, in the month of April, for determining the part that the Algiers Observatory will take in stellar photography. The direct image of the sun will be 6 centimetres in diameter. A spectroscope by Thollon will be put into operation. The extent of the spectrum will be 10 metres. M. Trépied has organised the electrical transmission of the time to the Hôtel de Ville of Algiers and Tunis. Colonel Perrier, head of the French and Algerian Survey, is arranging the measurement of the requisite triangles for connecting the Algiers Observatory with the Colonne Voirol, the starting-point of the Algerio-Tunisian system of triangulation.

MR. W. A. CARTER, of the Colonial and Indian Exhibition, writes to us that during this last spring he placed a specimen of the Mexican axolotl in an empty (? dry) réceptacle, where it has remained ever since. It is in a lively condition. The colour of the animal has become less intense, the gills have apparently disappeared, and the powers of locomotion seem quickened.

IT is worthy of note that at the establishment of the National Fish Culture Association many of the brook trout (*Salmo fontinalis*) hatched during February 1885 commenced to spawn last week, yielding about five hundred ova each. This fact is another proof of the extraordinary reproductive capacity of fishes in spite of age and artificial existence, for the fish in question have been maintained in a pond of limited dimensions. The size of the ova is small as compared with those of mature fish, therefore it is not likely that the trout when hatched will be large. The parents are in a healthy condition, and seem in no way weakened.

A CONSIGNMENT of nearly a thousand German carp of various kinds has arrived at the Colonial and Indian Exhibition. The great hardihood of the carp is evidenced by the fact that the fish in question were retained in carriers for sixty hours before being placed in tanks, when only two were found to have succumbed.

IN a paper in the October number of the *American Journal of Science* by Mr. O. W. Huntington, "On the Crystalline Structure of Iron Meteorites," the author concludes as follows:—"We have tried in this paper to establish the following points: (1) that many of the masses of meteoric iron in our collections are cleavage crystals, broken off probably by the impact of the mass against the atmosphere; (2) that these masses show cleavages parallel to the planes of all the three fundamental forms of the isometric or regular system, namely, the octahedron, the cube, and the dodecahedron; (3) that the Widmanstättian figures and Neumann lines are sections of planes of crystalline growth parallel to the same three fundamental forms of the isometric system; (4) that on different sections of meteorites Widmanstättian figures and Neumann lines can be exhibited in every gradation, from the broadest bands to the finest markings, with no break where a natural line of division can be drawn; (5) that the features of the Widmanstättian figures are due to the eliminations of incompatible material during the process of crystallisation. This investigation throws no new light upon the origin of meteorites, except so far as it strengthens the opinion that the process of crystallisation must have been extremely slow. The occurrence of large masses of native iron occluding hydrogen gas, and containing nickel, cobalt, phosphorus, sulphur, &c., implies a combination of conditions which the spectroscope indicates as actually realised in our own sun and in other suns among the fixed stars, and the most probable theory seems to be that these masses were thrown off from such a sun, and that they very slowly cooled, while revolving in a zone of intense heat. In this paper we have not taken into consideration a number of iron masses, whose meteoric origin has been generally accepted, which show no Widmanstättian figures, and not even any Neumann lines. A considerable proportion of these are certainly not meteoric. In the Harvard cabinet there are two specimens, labelled respectively Campbell County (Tennessee), and Hominy

Creek (North Carolina), which are evidently nothing but cast-iron, and a third, labelled Tarapaca Hemalga (Chili), which is probably of similar material. We could find on the specimens of this class in the Harvard collection no distinct evidences of crystallisation; but also we could find no features incompatible with that unity of structure which it has been the chief object of this paper to illustrate."

MR. HORATIO HALE has issued in pamphlet form his address "On the Origin of Languages and the Antiquity of Speaking Man," delivered before the Anthropological Section of the American Association for the Advancement of Science at Buffalo last August. The author's views were much discussed at the time, and those interested in the subject will be thankful to have them presented in this convenient form. Rejecting all the theories hitherto advanced by Lyell, Frederick Müller, and others, he endeavours to account for the vast number of *specifically distinct* languages spoken by races *not specifically distinct* by assuming that they originated from children's prattle in independent centres after the spread of speechless man over the globe. The cases are mentioned of the Boston twins born in 1860 and of some other "Geschwister," who appear to have evolved and practised for some time infantile jargons understood only amongst themselves, which it is argued might, under favourable conditions of isolation and so forth, develop into regular forms of speech consistently worked out with their own vocabularies and grammatical structure. In this way linguistic families differing absolutely one from the other need not be of any great antiquity, and in fact may have been developed from slight germs in many places and at different times since the dispersion of the "*homo alallus*" from some given centre. This *homo alallus* himself is admitted to be the lineal descendant of the men of the Stone Age, who are assumed to have been speechless, so that all forms of speech now current may be of comparatively recent date, say, not more than 8000 or 10,000 years, notwithstanding their great number and profound differences. This theory, which refers human speech in the first instance to "the language-making instinct of very young children," is presented with considerable force and plausibility, but will scarcely be taken seriously either by philologists or anthropologists. The latter especially will find it difficult to accept the conclusion that man properly so called, the *homo sapiens*, as distinguished from his precursor of the Neolithic Age, does not date further back than "somewhere between 6000 and 10,000 years ago." The theory also requires us to regard this first speaking man as already fully developed, possessing "intellectual faculties of the highest order, such as none of his descendants has surpassed," thus reversing the conclusions of modern anthropology.

IT is reported from Vienna that a great ice cavern has been discovered on the southern slope of the Dachstein, or Schneeberg, the very conspicuous lofty mountain in Lower Austria, which is visible from the ramparts of the capital. The general direction of the cavern runs from south to north, and it has been explored for a distance of 600 metres, a sharp precipice seemingly 14 metres deep having stopped for the time further progress. The cavern is from 5 to 6 metres broad, and very lofty, giving the impression that the ice is enormously thick. The explorers are of opinion that a subterranean lake will be found in the cavern.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Miss Edith Prowse; four Common Hedgehogs (*Erinaceus europæus*), British, presented by Mr. W. Walkinshaw; a —— Buzzard (*Buteo* ——) from Mogador, North Africa, presented by Mr. P. L. Forwood; a Ring-necked Parrakeet (*Palæornis torquatus* ♀) from India, presented by Mr. W. S. Bradshaw; an Aldrovandi's Skink (*Plestiodon auratus*) from North Africa, deposited; a Rusty-spotted Cat (*Felis rubiginosa*) from Ceylon, two Diuca Finches (*Diuca grisea*) from Chili, two Wood Larks (*Alauda arborea*), British, purchased; eight Long-fronted Gerbilles (*Gerbillus longifrons*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE BINARY STAR γ CORONÆ AUSTRALIS.—Mr. H. C. Wilson, of Cincinnati Observatory, has published elements of the orbit of this interesting southern double star in the *Sidereal Messenger* for October. These elements, which do not differ much from a set recently computed by Mr. Gore (*Monthly Notices*, vol. xlvi. p. 104), are as follows:—

$P = 78{\cdot}80$ years
$T = 1887{\cdot}40$
$e = 0{\cdot}324$
$\gamma = 50^{\circ}{\cdot}5$
$\lambda = 139^{\circ}{\cdot}0$
$\Omega = 41^{\circ}{\cdot}0$
$a = 1''{\cdot}85$

Comparing observations made 1834·47 to 1883·62 with this orbit, Mr. Wilson finds that the position-angles are well represented, with the exception of those observed by Powell from 1859 to 1864, which seem to be affected by systematic error, and thinks we may conclude the period is not far from eighty years. It is to be hoped that numerous observations of this star will be obtained during the next ten years, while the distance is small and the angular motion rapid.

OPPOLZER'S ASTRONOMICAL REFRACTIONS.—Herr Oppolzer has recently published, in the *Transactions* of the Mathematical and Natural Science Section of the Imperial Academy of Sciences of Vienna, vol. liii., a paper containing a theoretical discussion of the problem of astronomical refraction, followed by numerical tables intended to facilitate the practical application of the results at which he arrives. The relation between the temperature (t) and density (ρ) of the atmosphere which Herr Oppolzer adopts is

$$\frac{\delta t}{\delta \rho} = \epsilon + \Sigma k \rho^{\sigma-1},$$

where k and σ are quantities depending on the state of the atmosphere and on the place of observation. Whatever may be thought of the legitimacy of a relation of this form from a theoretical point of view, it at all events has the advantage, in Herr Oppolzer's skilful hands, of leading to a comparatively simple expression for the amount of refraction, deduced from a modification of the ordinary differential equation. And that it is capable, when the approximations are carried far enough, of giving results of great accuracy for large zenith distances, is shown by a comparison made between the computed values of the refraction and the well-known observations of Argelander, which form the basis of Bessel's supplementary table given in the "Tabulæ Regiomontanæ," with the following results:—

Z.D.		Observed—Computed
° ′		″
85 0	...	− 1·1
86 0	...	+ 1·2
87 0	...	− 1·3
88 0	...	− 2·5
89 0	...	+ 2·3
89 30	...	+ 1·8

COMETS FINLAY AND BARNARD.—The following ephemerides for Berlin midnight are from the *Astronomische Nachrichten*, No. 2752:—

Comet Finlay (1886 *e*)

1886	R.A.	Decl.	Log r	Log Δ
	h. m. s.	° ′		
Nov. 8	19 25 22	24 56·8 S.	0·0751	0·0970
10	33 49	24 36·5		
12	42 24	24 14·1	0·0718	0·0932
14	51 5	23 49·5		
16	19 59 51	23 22·7 S.	0·0697	0·0899

Comet Barnard (1886 *f*)

1886	R.A.	Decl.	Log r	Log Δ
	h. m. s.	° ′		
Nov. 7	12 7 8	8 18·5 N.	0·0735	0·2031
9	15 5	9 0·3		
11	23 31	9 44·3	0·0551	0·1772
13	32 29	10 30·7		
15	12 42 1	11 19·2 N.	0·0366	0·1507

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *NOVEMBER* 7-13

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 7

Sun rises, 7h. 6m.; souths, 11h. 43m. 49·6s.; sets, 16h. 22m.; decl. on meridian, 16° 21′ S.: Sidereal Time at Sunset, 19h. 29m.

Moon (Full on November 11) rises, 15h. 4m.; souths, 20h. 58m.; sets, 3h. 2m.*; decl. on meridian, 1° 49′ S.

Planet	Rises h. m.		Souths h. m.		Sets h. m.		Decl. on meridian ° ′
Mercury ...	... 9 22	...	13 11	...	17 0	...	23 59 S.
Venus ...	... 6 26	...	11 20	...	16 14	...	13 25 S.
Mars ...	... 10 42	...	14 27	...	18 12	...	24 27 S.
Jupiter...	... 4 55	...	10 19	...	15 43	...	7 43 S.
Saturn...	... 20 30*	...	4 32	...	12 34	...	21 18 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

Nov.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
7	4 Ceti	6	17 45	18 32	32 321
7	5 Ceti	6	18 0	19 1	48 311
7	B.A.C. 5	6	18 23	19 42	89 281
9	ν Piscium	4½	18 4	19 9	60 277
12	48 Tauri	6	19 18	20 18	61 251
12	γ Tauri	4	21 17	22 25	55 271
13	75 Tauri	6	2 38	3 37	162 275
13	θ¹ Tauri	4½	2 46	3 57	62 17
13	θ² Tauri	4½	3 6	near approach	39 —
13	B.A.C. 1391	5	3 39	4 46	115 332
13	Aldebaran	1	6 27	7 16	165 284

Saturn, Nov. 7.—Outer major axis of outer ring = 43″·5; outer minor axis of outer ring = 16″·8; southern surface visible.

Nov.	h.	
13 ...	17 ...	Mercury at greatest elongation from the Sun, 22° east.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
S Cassiopeiæ ...	0 17·1	55 10 N.	... Nov. 9,		*M*
U Cephei ...	0 52·2	81 16 N.	... ,, 8,	3 49	*m*
			,, 12,	3 29	*m*
Algol ...	3 0·8	40 31 N.	... ,, 11,	3 56	*m*
R Aurigæ ...	5 8·1	53 27 N.	... ,, 10,		*M*
S Cancri ...	8 37·4	19 27 N.	... ,, 9,	3 53	*m*
U Ophiuchi ...	17 10·8	1 20 N.	... ,, 8,	3 37	*m*
			and at intervals of	20 8	
β Lyræ ...	18 45·9	33 14 N.	... Nov. 12,	19 0	*m*
R Lyræ ...	18 51·9	43 48 N.	... ,, 13,		*m*
η Aquilæ ...	19 46·7	0 43 N.	... ,, 7,	5 0	*m*
R Vulpeculæ ...	20 59·3	23 22 N.	... ,, 9,		*M*
δ Cephei ...	22 24·9	57 50 N.	... ,, 10,	5 0	*M*

M signifies maximum; *m* minimum.

Meteor Showers

A radiant near δ Hydræ, R.A. 124°, Decl. 4° N., and one in Camelopardus, R.A. 102°, Decl. 73° N., are active in the early part of this week. Moonlight interferes with meteor observation during the greater part of the week.

THE HIGH TEMPERATURE IN OCTOBER

THE warm weather which occurred at the commencement of the month was so exceptional for the season, and extended over so large a part of Europe, that a few facts as to its general character may be of interest, and will afford opportunity of comparison with earlier records, as well as with records of any similar weather in time to come.

The highest temperatures were experienced during the first five days of the month, and were chiefly confined to Western, Central, and Southern Europe. During this time atmospheric pressure was generally high over Central Europe, and decreased towards the western or Atlantic coasts, so that the conditions of pressure were favourable to anticyclonic circulation over France and the south-east of England, and cyclonic circulation in Ireland and the northern parts of the British Islands. The barometric gradients were very slight over the Continent, but were rather steeper over Great Britain and Ireland, owing to the proximity of a barometric depression to the westward. This distribution of pressure was accompanied by southerly and south-easterly winds over Western Europe, and especially over France and our own islands, but it was only in Ireland and the more western parts of Great Britain that the wind was at all fresh.

At this season of the year our warmest weather in England is commonly experienced with south-easterly winds, as is well shown in the valuable discussion of the Greenwich observations for the years 1849 to 1868, in which the temperatures have been averaged for the several wind directions. The following are the temperatures for October:—

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
Monthly means	47	50	52	55	53	53	51	51
Highest hourly means	52	55	59	61	59	58	57	55

The same discussion also shows the striking difference which exists, in October, between the temperature with a cloudless and a cloudy sky:—

	Mean	Mean max.	Mean min.
Cloudless sky ...	50·8	61·1	43·5
Cloudy sky ...	51·8	54·8	49·8

The high temperatures experienced over England in October this year occurred with an exceptionally clear sky, as well as with a remarkably steady south-easterly wind, and the air before reaching England had been subjected to very similar conditions on the continent of Europe.

The following table gives the *maximum* day temperatures at twenty stations selected from the Daily Weather Report of the Meteorological Office and from the Paris *Bulletin International* for the first five days of October:—

	Station	Day 1	2	3	4	5	Mean
		°	°	°	°	°	°
British Islands	York ...	64	63	66	69	63	65
	Greenwich ...	78	68	69	79	77	74
	Parsonstown (Ireland)	58	61	61	64	66	62
France	Dunkirk ...	79	72	72	81	79	77
	Cherbourg ...	72	73	61	72	68	70
	Paris ...	77	65	78	78	77	75
	Nantes ...	79	70	81	82	64	75
	Biarritz ...	82	73	81	81	68	77
	Nice ...	72	73	75	77	75	74
Germany	Hamburg ...	61	68	61	64	68	64
	Berlin ...	63	73	63	61	66	65
	Carlsruhe ...	75	70	75	72	70	72
Belgium—	Brussels ...	76	64	71	75	77	73
Austria—	Vienna ...	73	75	73	72	70	73
Spain and Portugal	Barcelona ...	88	100	95	91	85	92
	Madrid ...	62	72	75	70	68?	69
	Lisbon ...	70	68	68	70	68	69
Italy	Turin ...	72	73	73	73	73	73
	Rome ...	77	79	81	81	81	80
	Palermo (Sicily) ...	82	84	84	82	84	83
	Mean ...	73	72	73	75	72	

The stations have been selected as representative of Western, Central, and Southern Europe, and the table shows well the area over which the warm weather extended.

The more northern parts of Europe did not experience any exceptional heat, the highest temperature at Copenhagen being 63°, and at Stockholm 61°. The more western parts were also but little affected: in Ireland the highest maximum was 66° at Parsonstown on the 5th, and at no other station was the temperature above 65°. In Scotland the temperature did not reach 70°.

The Greenwich observations from 1841 show that a higher temperature has only once been registered in October, viz. 81° on the 4th in 1859; but the daily mean, which was 67°·1 on the 4th this year, is higher than any previously recorded.

The observations which were made in the apartments of the Royal Society from the year 1794, excepting the years 1811 to 1819, do not show so high a reading between 1794 and 1840. At Kew Observatory the highest temperature recorded was 77°

on the 4th, and this is the highest ever observed in the month of October; on the 5th, 76° was registered, which corresponds with the temperature observed on October 4, 1859. The returns of the Meteorological Office show that 80° was observed on the 4th in London and at Cambridge, whilst 77° was registered at several stations in the east of England and in the Midland Counties.

It is difficult to make any satisfactory comparison with previous records, except at one or two places, but these tend to show that so high a temperature at this season does not occur more than about twice in a century. CHAS. HARDING

VOLCANOES OF JAPAN

THE last number (vol. ix. part 2) of the *Transactions* of the Seismological Society of Japan is wholly occupied by a paper of Prof. Milne's, on Japanese volcanoes, which is the longest contribution that has yet appeared in the Society's *Transactions*. The paper is partly historical and partly scientific, and contains, so far as the writer has been able to collect, references to everything that is known on the subject. Very much comes from his own observations, for he has travelled over the greater part of Japan, and has ascended many of the volcanoes. The paper also contains an epitome of some thirty or forty works in Japanese. On the whole, it is a systematic account of material which has been accumulating for the last eleven years.

The following are the more important conclusions which Prof. Milne has formulated in the paper:—

1. *Number of Volcanoes.*—As Japan has not yet been completely explored, and, moreover, as there is considerable difficulty in defining the kind of mountain to be regarded as a volcano, it is impossible to give an absolute statement as to the number of volcanoes in the country. If under the term volcano be included all mountains which have been in a state of eruption within the historical period, those which have a true volcanic form, together with those which still exhibit on their flanks matter ejected from a crater, we may conclude that there are at least 100 such mountains in the Japanese Empire. If to this list be added the ruins and basalt wrecks of volcanic cones, the number would be considerably increased. These mountains are distributed as follows:—

Northern Region..	Kuriles	23
	Yezo	28
Central Region ...	Northern main island Central " " Oshima group	35
Southern Region..	Southern main island ...	1
	Kiushiu Southern islands	13
	Total	100

Of this number about 48 are still active, or have been so during the historical period. These active volcanoes are distributed as follows:—

Northern Region..	Kuriles	16	27
	Yezo	11	
Central Region			12
Southern Region			9
	Total		48

From this it will be seen that volcanic activity in Japan decreases from the north towards the south.

2. *Number of Eruptions.*—Altogether about 232 eruptions have been recorded, and of these the greater number took place in the southern districts. This may perhaps be accounted for by the fact that Japanese civilisation advanced from the south. In consequence of this, records were made of various phenomena in the south when the northern districts were still unknown and unexplored regions. The greater number of eruptions took place in February and April. Comparing the frequency of eruptions in the different seasons, the volcanoes of Japan appear to have followed the same law as the earthquakes, a greater number having taken place during the cold months. This winter frequency of volcanic eruptions may possibly be accounted for in the same manner that Dr. Knott accounted for the winter frequency of earthquakes. During the winter months the average barometric gradient across Japan is steeper than in summer. This, coupled with the piling up of snow in the northern regions, gives rise to long-continued stresses, in consequence of which certain portions of the earth's crust are more prepared to give way during the winter months than they are in summer.

3. *Position and Relative Age of Japanese Volcanoes.*—The youngest of the Japanese volcanoes appear to be those which exist as, or on, small islands. On the islands in the Kuriles, in the Oshima group, and in the Satsuma sea, many of the volcanoes are yet young and vigorous. Moreover, many of these islands have been formed during the historical period. The island-forming period in the Satsuma sea, for example, was about the year 1780.

The volcanoes of Japan form a long chain running from N.E. towards S.W.; but a closer examination of the distribution of the volcanic vents shows that there are probably four lines:—

(*a*) The N.E.-S.W. line running from Kamchatka through the Kuriles and Northern Yezo.

(*b*) The curved line following the backbone of the main island, and terminating on the western side of the Yezo anticlinal.

(*c*) The N.N.W.-S.S.E. line of the Oshima group. This line, coming from the Ladrones, passes through Oshima and Fujisan parallel to and near to the line of a supposed fault. Here it intersects the main line running through the main island. Volcanic vents are here very numerous. As the main island line is intersected, while the Oshima line is the intersector, it may be argued that the Oshima-Fujisan line of volcanoes are younger than many of those on the main island line.

(*d*) The Satsuma line, coming from the Philippines through Sakurajima and culminating in the famous Mount Aso, which is the nucleus of Kiushiu.

4. *Lithological and Chemical Character of Lavas.*—Although Prof. Milne has made an extensive collection of the volcanic rocks of Japan, the opportunity for examining them has not yet presented itself, and therefore he can only speak of them in general terms. They are at present being carefully studied by the officers of the Geological Survey. The rocks in his possession are chiefly andesites. Those containing augite, like the rocks of Fujisan, closely approximate to basalts. True basalt is, however, rare. Another common rock is hornblende andesite, some of which contains free quartz. Quartz trachytes occur in the north of Japan. The following table shows the percentages of silica, and ferrous and ferric oxide, contained in the rocks of ten volcanoes:—

Locality.	SiO_2	FeO	Fe_2O_3
1. Norokura	61·72	1·35	3·50
2. Misake	59·97	3·27	3.86
3. Kusatsu	61·49	3·30	4·35
4. Amagi (Hakone)	65·34	2·45	3·09
5. Komagadake	56·27	2·19	6·69
6. Moriyoshi	59·17	2·65	4·15
7. Chokai	60·64 54·55	3·81 5·19	3·14 4·42
8. Hakone (Tonosawa)	48·97	4·02	4·81
9. Fujisan	49·00	5·1	6·06
10. Oshima	52·00	13·70(?)	

One feature exhibited by the table is that the rocks of Oshima, Fujisan, and Tonosawa are basic, while those like Chokaisan and Moriyoshiyama belonging to the line of volcanoes of the main island, are relatively acidic. More extended observations of this description may show that different lines of volcanoes have thrown out different lavas, or that the lavas of different constitution are of different ages.

5. *Magnetic Character of Rocks.*—In a study of the soils in the neighbourhood of Tokio, Mr. E. Kinch refers specially to the magnetite they contain. A great portion of this comes from the disintegration of volcanic rocks. Many of the Japanese lavas have a distinct effect upon a compass needle, and many of the black lavas from the crater of Fujisan will easily turn the needle of an ordinary compass through 360°. Many of the pieces of lava are not only magnetic but polar. Dr. Naumann found a block of augite trachyte on the top of Moriyoshiyama which would deflect the needle of a compass through 155°. The most curious observation made by this investigator was that the magnetic declination near Gaujusan has during the last eighty years (when it was about 14° 30′ E.) decreased 19°, being now about 5° W. As we recede from this mountain the amount of

change is less. Assuming this result to be correct, it would seem justifiable to look to Gaujusan as connected with these local changes. Some of the volcanoes in the Kuriles are said to exert a marked influence upon the compasses of ships. When a vessel is lying near certain mountains, as, for instance, in Bear Bay at the north end of Iturup, a distant mountain will have a very different bearing to that which is indicated by the same compass when the vessel is a short distance outside Bear Bay. In both cases the ship may be lying in the same direction, and the direction of observation is practically along the same line. This leads Prof. Milne to urge, as he has already done, that a magnetic observatory should be placed on or near one of the nine active volcanoes of Japan. Changes in volcanic activity are probably accompanied by local changes in the magnetic effects produced by subterranean volcanic magmas. These changes may be due to alterations in position, alterations in chemical constitution, and changes due to the acquisition or loss of heat. If such is the case, he argues, the records of a magnetic observatory would lead up to a knowledge of the changes taking place beneath the ground. When it is remembered that volcanoes like Oshima (Vries Island), where it seems probable that there may be local and rapid changes in magnetic variation taking place, lie in the track of so many vessels, the proposed investigation has a practical as well as a scientific aspect. An investigation of earth-currents at and near volcanoes might be added to the magnetic investigations.

6. *Intensity of Eruptions.*—It appears from the accounts of eruptions which are given in the paper that the intensity of volcanic action in Japan has been as great as in any other part of the world. One period of unusual activity was between the years 1780 and 1800, a time when there was great activity elsewhere in the globe. It was during this period that part of Mount Unsen was blown up, and from 27,000 to 53,000 persons (according to different accounts) perished, that many islands were formed in the Satsuma sea, that Sakurajima threw out so much pumice material that it was possible to walk a distance of 23 miles upon the floating *débris* in the sea, and that Asama ejected so many blocks of stone—one of which is said to have been 42 feet in diameter—and a lava-stream 68 kilometres in length.

7. *The Form of Volcanoes.*—The regular so-called conical form is very noticeable in many of the Japanese mountains, especially perhaps in those of recent origin. Outlines of these volcanoes, as exhibited either by sketches or photographs, show curvatures which are similar to each other. From a collection of photographs Prof. Milne traced the profiles of a number of important mountains in Japan. These are reproduced in the paper (see Fig. 1). From an examination of these figures he found that the curvature of a typical volcano was logarithmic, or, in other words, the form of such a mountain was such as might be produced by the revolution of a logarithmic curve round its asymptote. In his original paper on the subject he said that the form agreed with that which would be produced by the piling up of loose material. He ought to have said it was the form assumed by a self-supporting mass of coherent material. Mr. George F. Becker (*American Journal of Science*, October 1885) continues these observations by an analytical investigation of the conditions of such equilibrium. If the height of a column is a, its radius y, the distance of any horizontal plane from the base x, the specific gravity of the material ρ, and the co-efficient of resistance to crushing at the elastic limit k, then the equation of the curve, which by its revolution about the x axis will generate the finite unloaded column of the "least variable resistance" is—

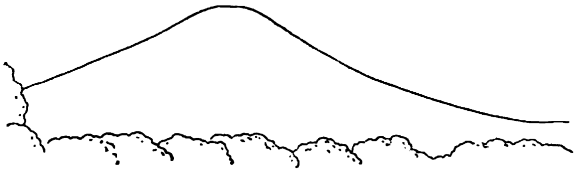

FIG. 1.—Outline of Fujiyama, from a photograph. This may be taken as typical of many Japanese volcanoes.

$$\frac{y}{c} = \frac{e^{-\frac{x}{c}} - e^{\frac{x}{c}}}{2},$$

where

$$c = \frac{2k}{\rho}.$$

This latter quantity is of course different for different materials. It can be expressed in terms of x and y—

$$\frac{2k}{\rho} = \frac{y}{(\tan^2 d - 1)^{-\frac{1}{2}}},$$

d being the angle which the tangent at any point makes with the x axis. The value of c can be obtained from photographs or drawings of a mountain, while ρ may be obtained from pendulum experiments or from specimens of volcanic material. With these data we can determine the modulus of resistance at the elastic limit of the materials which compose a mountain on a large scale for many constituents of the earth's crust. Mr. Becker concludes his observations by remarking that a study of the form and dimensions of lunar volcanoes would lead to values of $\frac{k}{\rho}$, from whence we might approximately determine whether the lunar lava is similar to that of terrestrial origin. In the table which follows, Prof. Milne has followed out Mr. Becker's suggestion, and calculated the modulus of resistance to crushing at the elastic limit in pounds per square foot for a number of Japanese mountains. The different values for $\frac{2k}{\rho}$ for the same mountain is in great measure due to the absence of an accurate scale for the various photographs which had to be investigated. Another difficulty was obtaining a value for r, or the density of the mountain. Prof. Mendenhall, who made a number of experiments with pendulums on the summit of Fujisan, says the rocks of that mountain have a density of 1·75. This is when they have air in their pores. As powder the density becomes 2·5. Wada gives the specific gravity of the rock on Fujisan as 2·6.

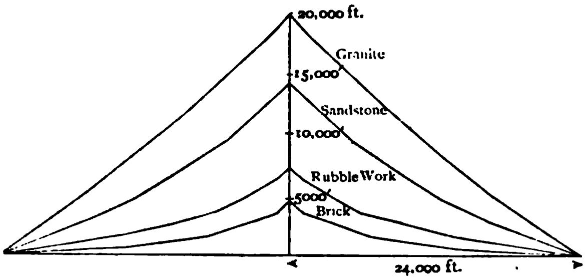

FIG. 2.—Theoretical Mountains.

Assuming the density of the earth at 5·67, then the density of Fujisan, as determined by Prof. Mendenhall's experiments, is 2·08. In the following table the density of the materials of all the mountains mentioned is taken at 2·5.

	Height in feet	$\frac{2k}{\rho}$	$\frac{k}{\rho}$	Load in lbs. per square foot	
Fujisan	12,441	4200	...	...	Photograph
	...	5000	...	...	,,
	...	4240	...	...	,,
	...	3500	...	...	,,
	...	5420 5450	...	...	,,
	...	5440	...	...	,,
	...	3945 4133	...	...	,,
	...	4430	...	...	Surveyed section
	...	3640	...	...	,, ,,
Average for Fujisan	...	4490	2245	350,220	,, ,,
Iwakisan	5260	2360	1180	174,080	Photograph
Nantaisan......	3800[1]	2000	1000	156,000	,,
Alaid............	7773	2195 2120	1078	163,168	,,
Krakatão	2745	1310	655	102,180	Surveyed section
	...	1310			

Comparing the results given in the above table with the numbers given in the next section, which are based on experi-

[1] This is the height above Lake Chuzenji.

ments referred to in Rankine's "Civil Engineering," it may be said that the average strength of Fujisan lies between that of rubble work and sandstone; Iwakisan, Nantaisan, and Alaid are like good rubble masonry, while the strength of the ill-fated Krakatão is not much above that of ordinary brickwork.

8. *Theoretical Mountains.*—As it might be interesting to compare actual mountains with theoretical mountains constructed from the equation—

$$y = \frac{c}{2}\left(e^{\frac{n}{c}} - e^{-\frac{n}{c}}\right)$$

such mountains have been drawn, and are shown in Fig. 2. The values of c are given in the following table.

In drawing up the table the instantaneous breaking strength of granite and its crumbling strength, which is the largest possible value for k, are taken as being equal. For sandstone the crumbling strength is assumed to be three-fourths of the breaking strength, while for rubble work and brickwork it has been taken as one-half.

Material	Instantaneous breaking strength in lbs. square feet	Crumbling strength or k in lbs.	Weight cubic foot lbs.	$c = \frac{2k}{\rho}$
Granite	1,584,000	1,580,000	170	18,500
Sandstone	790,000	590,000	144	8,200
Rubble masonry	316,000	150,000	120	2,500
Brickwork	144,000	72,000	112	1,300

The diameter of the base of each of these mountains is 48,000 feet, and the height to which mountains of the following different materials could be built upon such a base without crushing would approximately be:—

Brickwork	4,600 feet	Sandstone	14,500 feet
Rubble masonry	7,300 „	Granite	20,000 „

9. *Causes Modifying Volcanic Forms.*—Causes modifying the natural curvature of a mountain are:—

(1) The tendency during the building up of the mountain of the larger particles to roll farther down the mountain than the smaller particles.

(2) The effects of atmospheric denudation, which carries materials from the top of the mountain down towards the base.

(3) The position of the crater, and the direction in which the materials are ejected.

(4) The existence of parasitic craters on the flanks of a mountain.

(5) The direction of the wind during an eruption.

(6) The sinking of a mountain in consequence of evisceration beneath its base.

(7) The expansions and contractions at the base of a mountain due to the acquisition or loss of heat before and after eruptions.

10. *Effect of Volcanic Eruptions on the People.*—The eruptions in Japan from time to time have exerted a very marked influence upon the minds of the Japanese people. Divine interference has been sought to prevent eruptions, priests have been ordered to pray, taxes have been repealed, charities have been instituted, special prayers against volcanic disturbances have been formulated, and have remained in use for the period of 100 years, while special days for the annual offering up of these prayers have been appointed. At the present day a form of worship to mountain deities is not uncommon.

SOLUTION[1]

Opening of the Discussion by Prof. Tilden

FOR want of time, the consideration of various phenomena connected with the subject was necessarily omitted. Thus no reference could be made to the various formulæ relating to expansion or density of solutions, nor to their optical properties, magnetic rotation, nor to the subject of electrolysis. In what follows, a review is presented of the principal phenomena observed in the act of solution of solids (especially metallic salts and other comparatively simple compounds) in liquids, and the chief properties of the resulting solutions, with the object of arriving (if possible) at some conclusion as to the physical explanation of the facts. The question must at once arise whether these phenomena are to be considered as chemical or mechanical, and all the theories which have been put forward to explain the nature of solution are roughly divisible into two classes, according as, on the one hand, they represent the process as a kind of chemical combination, or, on the other, explain the phenomena by reference to the mechanical intermixture of molecules, or by the influence of the rival attractions of cohesion in the solid and liquid, and of adhesion of the solid to the liquid. The former hypothesis seems to have been universally adopted by the older writers, such as Henry and Turner, and it seems pretty clear that Berthollet also regarded solution as an act of chemical combination. Among modern chemists, Prof. Josiah P. Cooke takes a similar view, but M. Berthelot is the most consistent and powerful supporter of the same hypothesis. In his "Mécanique Chimique," tome ii. p. 160, will be found a very clear and formal statement of the views upon this subject which, it is interesting to know, are retained by M. Berthelot without modification in any essential particular.

On the other hand, there are a number of writers who, whilst referring the phenomena of solution to a molecular attraction of some kind, do not attribute solubility to the formation of chemical compounds of definite composition. Graham distinctly ranges himself on this side. Brande also appears to have taken a similar view; Daniell, Miller, Nicol, and Dossios may be more or less ranked with them. A theory differing in some important respects from those of the above writers was briefly enunciated in a paper communicated to the Royal Society by Tilden and Shenstone in 1883. In discussing the connection between fusibility and solubility of salts, the authors point out that the facts tend to "support a kinetic theory of solution, based on the mechanical theory of heat. The solution of a solid in a liquid would accordingly be analogous to the sublimation of a solid into a gas, and proceeds from the intermixture of molecules detached from the solid with those of the surrounding liquid. Such a process is promoted by rise of temperature, partly because the molecules of the still solid substance make longer excursions from their normal centre when heated, partly because they are subjected to more violent encounter with the moving molecules of liquid." This theory, however, only relates to the initial stage of the process of solution, and does not sufficiently explain saturation nor the influence of dissolved substances upon vapour-pressure, specific heat, specific volume, &c. How far is it true that evolution of heat indicates chemical combination: does the evolution of heat which often takes place on dissolving a solid in water, or on adding more water to its solution, indicate the formation of hydrates, *i.e.* compounds of the dissolved body with water in definite proportions? Thomsen answers this question in the negative ("Thermochemische Untersuch.," Band iii. p. 20).

Take the case of sulphuric anhydride (SO_3). It is evident from the diagram exhibited that more than half the total evolution of heat occurs on addition of the first molecule of water to the solid substance; yet the succeeding molecules give quite an appreciable thermal change. At what point in such a curve should we be justified in setting up a distinction between the effect due to chemical combination and that due to other causes, such as the change of volume consequent on dilution or the possible loss of energy from the adjustment of the motion of the molecules of the constituents to the conditions requisite for the formation of a homogeneous liquid, or (though not in the present case) the decomposition of the compound by the water? In the act of solution of the solids, and especially of anhydrous salts in water, the volume of the solution is always less than the sum of the volumes of the solid and its solvent, with the exception of some ammonium salts in which expansion occurs. Similarly the addition of water to a solution is followed by contraction. This contraction may be due to mere mechanical fitting of the molecules of the one liquid into the interspaces between the molecules of the other (see Mendelejeff's abstract in *Journ. Chem. Soc.*, Feb. 1885, p. 114). This would probably not be attended by loss of energy. Or the contraction may arise from the readjustment of molecular motion already referred to.

If we know the coefficient of expansion of the liquid and its specific heat, we can calculate the amount of heat evolved for a given contraction. If this is done for sulphuric acid, and many other cases, it is found that, after accounting for the thermal change due to alteration of volume alone, there is a surplus of heat evolved which may really indicate some kind or some amount of chemical combination.

Thomsen has found that as a rule the heat of solution and of dilution are both either positive or negative. Of thirty-five salts examined, only four supply well-marked exceptions. However we may ultimately explain the anomaly exhibited by these salts, the fact remains that the heat evolved or absorbed during the admixture of any substance with water is in every case a continuous function of the quantity of water added. Similarly the contraction which ensues on diluting an aqueous solution proceeds continuously, and the molecular volume of a salt in solutions of different strengths is continuously greater the larger the amount of salt present. So that in none of these thermal or volumetric phenomena is any discontinuity observed, or any indication of the formation of compounds of definite composition, distinguishable by characteristic properties.

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[1] Report of a discussion at the Birmingham meeting of the British Association.

The question we are now considering, as to whether in a solution the solvent and the substance dissolved in it—or any portion thereof—exist independently of each other, is in some degree answered by the facts known as to the specific heats and vapour-pressures. For instance, when water is added to a solution of sodium nitrate, the molecular heat of the resulting liquid seems to show that all the water added is influenced at least until a very large quantity is present. In this case one molecule of sodium nitrate can affect the movements of a hundred molecules of water, and probably more. It is also well known that the vapour-pressures of water holding in solution almost any dissolved solid is less than the vapour-pressure of pure water, and that the boiling-point of a liquid is raised by the addition to it of any soluble non-volatile substance. This fact of reduction of pressure can only be explained upon the hypothesis that there is no free water present at all; that is, that there is no water present which is not more or less under the influence of the dissolved substance.

What becomes of water of crystallisation forms a part of the same question as to the relation of solvent to solvent. Observed facts lead us to conclude that white copper sulphate, blue anhydrous cobalt chloride—and, by analogy, other salts which are colourless—retain their hold upon water of crystallisation when they are dissolved in water. A very important observation has been made by Dr. Nicol which bears directly upon this question. In his study of the molecular volumes of salt solutions he finds that, when a salt containing water of crystallisation is dissolved, this water is indistinguishable by its volume from the rest of the water of the solution. In the report presented to the British Association last year, the following passage occurs: "These results point to the presence in solution of what may be termed the anhydrous salt in contradistinction to the view that a hydrate, definite or indefinite results from solution; or in other words, no part of the water in a solution is in a position relatively to the salt different from the remainder."

These two statements, however, are not strictly consequent upon each other. The view seems preferable that (save, perhaps, in excessively dilute solutions) the dissolved substance is attached in some mysterious way—it matters not whether it be supposed to be chemical or physical—to the *whole* of the water. We cannot otherwise get over the difficulty presented by the hydrated salts, which give coloured solutions, by the control of the vapour-pressure of the dissolved salt, and by the altered specific heat. With regard to water of crystallisation, E. Wiedemann has shown that hydrated salts in general expand enormously at the melting-point; and the observations of Thorpe and Watts on the specific volume of water of crystallisation in the sulphates of the so-called magnesium group show that, whilst the constitutional water occupies less space than the remaining molecules, each successive additional molecule occupies a gradually increasing volume. So that when a salt, with its water of crystallisation, passes into the liquid state (either by melting or by solution in water), it requires a very slight relaxation of the bonds which hold the water to the salt for it to acquire the full volume of liquid water, whilst the water of constitution is not so easily released. And this conclusion accords with Nicol's observations on the molecular volumes of the salts when in solution.

Now comes the question as to what determines the solubility of a substance. Why, for example, is magnesium sulphate very soluble in water, whilst barium sulphate is almost totally insoluble? With regard to salts the following propositions seem to be true:—(1) Nearly all salts which contain water of crystallisation are soluble in water, and for the most part are easily soluble; (2) insoluble salts are almost always destitute of water of crystallisation and rarely contain the elements of water; (3) in a series of salts containing nearly allied metals the solubility, and capacity for uniting with water of crystallisation generally, diminish as the atomic weight increases.

The fusibility of a substance has also much to do with its solubility. Neither fusibility alone nor chemical constitution alone seems to be sufficient to determine whether a solid shall be soluble or not. But it may be taken as a rule to which there are no exceptions that when there is a close connection in chemical constitution between a liquid and a solid, and the solid is at the same time easily fusible, it will also be easily soluble in that liquid.

Salts containing water of crystallisation may be considered as closely resembling water itself, and these are for the most part both easily fusible and easily soluble in water. But space is wanting for the discussion of the details of these matters, as well as of the relation of molecular volume to fusibility of solids.

The fascinating character of the phenomena of supersaturation has attracted a host of experimenters, but no definite explanation has been generally accepted. In the opinion of the speaker supersaturation is identical with superfusion. Supersaturated solution of, say, alum, thiosulphate of sodium melted in its water of crystallisation, and fused sulphur at 100°, exhibit phenomena of exactly the same kind.

Finally, we are led to the consideration of what is meant by chemical combination. From the phenomena under discussion, and others, the conclusion seems inevitable that chemical combination is not to be distinguished by any absolute criterion from mere physical or mechanical aggregation; and it will probably turn out ultimately that chemical combination differs from mechanical combination, called cohesion or adhesion, chiefly in the fact that the atoms or molecules of the bodies concerned come relatively closer together, and the consequent loss of energy is greater.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Of the students in Natural Science entered at Cambridge this term no fewer than 116 have already announced their intention of studying medicine.

DUBLIN.—The Senate of the Royal University has conferred the degree of Doctor of Science *honoris causâ* upon James Bell, Ph.D., F.R.S., Principal of the Somerset House Laboratory.

SCIENTIFIC SERIALS

Revue d'Anthropologie, troisième série, tome 1, Paris, 1886.—On the Simian characters of the Naulette jaw, by M. Topinard. This celebrated find, which was discovered at the bottom of an obscure cavern 25 m. below the present level of the Lesse, near Dinant, in Belgium, is chiefly remarkable for its excessive prognathism, which is due alike to the great thickness of the horizontal branch of the jaw when compared with its height, and to the special obliquity of the axis of the alveolus of the second molar. In its relative proportions the Naulette jaw must be characterised not only as non-human, but as plus-Simian. A careful comparison of the Naulette jaw with the maxillary processes of the anthropoids, and of several of the lowest extant human races, has led M. Topinard to the conclusion that in the age of the mammoth, tichorine rhinoceros, and cave-bear, there had already appeared numerous mixed human types, to one of the lowest of which it may be presumed that the Naulette jaw belonged.—On the population of Bambouk, on the Niger, by Dr. Colin. An interesting paper on an extensive, but very imperfectly-known, region of Western Soudan, exclusively inhabited by a branch of the great Manding race, known as the Mali-nkès. The Bambouk territories, more than 600 kilometres in length, and from 80 to 150 in width, are divided into numerous little States, most of which enjoy a complete autonomy. Their want of consolidation, and the indifference of the people to all forms of religion, have made the Mali-nkès objects of contempt to their Mussulman black neighbours, but according to the narrations of the Griotes, or itinerant bards, who are to be met with in every part of Western Africa, they had at one time extended their dominion over all the tribes on the right banks of the Niger, and were preparing to invade Saigon when the advance of the French forced them to fall back within their original limits. For a time they submitted to the restrictions of Mohammedanism, but now they appear to have absolutely no religion. They prepare an intoxicating drink from honey, called "dolo," in which women as well as men indulge to excess. The men are indolent, hunting only to avert starvation, and working their extensive gold-mines imperfectly, and chiefly by the help of the women, to whom falls the chief share of providing for the wants of the community, but who, after marriage, enjoy great freedom, although the young girls are kept under strict supervision.—On the human bones found in France in caverns belonging to the Quaternary age, by M. Cartailhac. Of such finds, none can be referred to the early period of the Saint Acheul, or Chelles deposits, the oldest belonging apparently to the Mousterian age, while the most abundant human remains are found in the comparatively recent beds of Solutré and La Madelaine. The former of these are remarkable for the enormous number of horse-bones accumulated about the stone hearths and in the kitchen-middens of this station. According to Dr. Cartailhac, 40,000 skeletons might be reconstructed from these equine remains, which seem to have been exposed to the action of fire, the greater number of the bones having been broken for the extraction of the marrow, whence he assumes that the horse must have reached its maximum development and served in the place of all other game at the period of the Solutré deposits. The writer compares together the human and other remains found in various Mediterranean and inland caves, with the special object of ascertaining how far the condition and mode of deposition of the skeletons can throw light on the vexed question whether the great preponderance of fractured over whole bones in these primæval graves indicates the practice of cannibalism, or whether it may not be dependent on the observance of special modes of burial, involving the burning or dismemberment of the body after death.—The facial angle proposed by Cuvier and Geoffroy Saint-Hilaire for comparative anatomical determinations and for measuring facial differences in the living subject, by Dr. Collignon. The writer, who considers at length the merits of the various angles proposed by Camper and others, concludes by showing the superiority, for practical purposes, of adopting Cuvier's facial angle, measured by Topinard's goniometer for determining the median angle.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, October 26.—M. Jurien de la Gravière, President, in the chair.—On the unequal flow of gases, by M. Haton de la Goupillière. In continuation of his recent communication on this subject the author here deals with the reverse problem of a receptacle originally filled with compressed air discharging itself freely into the atmosphere.—On the intensity of the magnetic field in dynamo-electric machines, by Marcel Deprez. Assuming that the most important element of a dynamo-electric machine, whether employed as a generator or receiver, is the magnetic field, the author deals with the influence of the deviation of the magnetic pieces, and shows that, contrary to the opinion of certain electricians, the intensity of the field decreases far less rapidly than the distance of the magnetic pieces increases. The influence of the dimensions perpendicular to the lines of force is also considered.—Researches on the decomposition of the bicarbonate of ammonia by water, and on the diffusion of its components through the atmosphere, by MM. Berthelot and André. From the experiments here described, the authors are led to the conclusion that it is the diffusion of the carbonic acid that determines the decomposition by water of the bicarbonate of ammonia, and consequently the transport of the ammonia itself. These results are of the greatest importance even for the purely physical study of the circulation of gases between the ground, the waters, and atmospheric air, apart altogether from the phenomena of vegetation.—Note accompanying the presentation of his work entitled "An Introduction to the Study of the Human Races," by M. de Quatrefages. This is the first volume of the "Bibliothèque d'Ethnologie," edited jointly by the author and M. Hamy. It contains a summary of the views expounded in greater or less detail in his other writings, while dealing more fully with a number of other matters, which he had hitherto merely indicated, or else entirely neglected for lack of the fresh data and discoveries which now enable him to discuss them seriously. One of the most important is the question of prehistoric man, and he now shows that even in Quaternary times the human race had already spread over the whole earth to the remotest extremities of the Old and New World. This ubiquity of Quaternary man already suggested the existence of the species in the previous epoch, and direct proofs of this fact have recently been multiplied to such an extent that the presence of man in Europe during Tertiary times may now be regarded as placed beyond reasonable doubt, although his presence in America is not yet established. The results yielded by palæontology, geology, and even history point to the extreme north of Asia as the cradle of the human race and the centre of dispersion, which had already begun in Tertiary times. Here also were differentiated the three fundamental types, to which all races may still be reduced, as well as the three linguistic types diffused throughout the globe. It is further shown that hypsistenocephaly is the main feature distinguishing the American from the European primitive race, and that the man of Canstadt, hitherto regarded as the oldest Quaternary type, in reality dates back to the Tertiary epoch.—Note on the meteorite which fell on January 27, 1886, at Nammianthul, in the Presidency of Madras, by M. Daubrée. This meteorite, a specimen of which has been received from Mr. Medlicott, of the Indian Geological Survey, presents the ordinary characters of the group of small sporadic asters.—Experiments on the transmission of force by means of a series of dynamo-electric machines coupled together, by M. Hippolyte Fontaine. These important experiments (carried out with seven Gramme machines, under the inspection of the Commissioners, MM. Bertrand, Becquerel, Cornu, Maurice Lévy, Marcel Deprez, and Mascart) show that it is possible to transmit an effective force of fifty horse-power through a resistance of 100 ohms at a loss of less than 50 per cent.—On algebraic surfaces capable of a double infinity of birational transformations, by M. E. Picard. In supplement to his previous communication on algebraic surfaces, the author here shows that, for all surfaces capable of a double infinity of birational transformation, the co-ordinates of any given point are expressed by the uniform (Abelian) functions of two parameters.—On the transformation of surfaces in themselves, by M. H. Poincaré. It is shown in connection with M. Picard's theorem that, in certain cases, the Abelian functions may degenerate into triply periodical, elliptical, or even rational functions.—Extension of Riemann-Roch's theorem to algebraic surfaces, by MM. Noether.—On the recomposition of white light by means of the colours of the spectrum, by M. Stroumbo. A process is described by means of which the recomposition of white light is effected, taking as the starting-point the very colours of the spectrum, and utilising, as in Newton's experiment with the disk, the persistence of the images on the retina.—Note on the principal showers of shooting-stars and the aurora borealis, by M. Ch. V. Zenger. A careful study of M. Rubenson's great Catalogue of the Auroras from 1800 to 1877 has unexpectedly revealed the fact that August 10 and November 14 show a great frequency of these lights, thus coinciding with the periods of the shooting-stars and suggesting a connection between these two orders of phenomena.—Influence of the amplitude of the lunar oscillation in declination on the shiftings of the northern trade-winds, by M. A. Poincaré. A study of the tables for 1880 83 shows certain relations between these phenomena, which, however, differ greatly according to the seasons.—On the phenomena associated with the heating and cooling of molten steel, by M. Osmond. It is shown that, as the quantity of carbon is increased, the temperature of transformation of the iron is lowered, and that of recalescence raised, so that both coincide in the hard steel.—Saturation of normal arsenic acid by the water of baryta, by Ch. Blarez.—On the function of the semicircular canals of the inner ear, by M. Yves Delage. The chief function of this apparatus, as already recognised by Goltz, Flourens, and others, is shown to be distinct from that of the auditory sense, and connected rather with the rotatory movements of the head, either alone or with the body.—On Syndesmis, a new type of Turbellariæ described by W. A. Sillimann, by M. Ph. François. This organism is shown to be, not an ectoparasite of the large green nematoid, as supposed by Sillimann, but a true endoparasite of *Strg. lividus*.—On two Synascidians new to the French sea-board (*Diazona hebridica*, Forbes and Goodsir, and *Distaplia rosea*, Della Valle), by M. A. Giard.—Organisation of *Lepidomenia hystrix*, a new type of Solenogaster, by MM. Marion and Kowalevsky.—On the Gephyrians belonging to the family of the Priapulidæ collected by the Cape Horn Mission, by M. Jules de Guerne. The discovery of these organisms is a remarkable instance of the presence in the southern seas of forms almost identical with those of the Arctic Ocean.—The simple epidermis of plants considered as a reservoir of water, by M. J. Vesque.—Remarks on *Poroxylon stephanense*, by MM. C. Eg. Bertrand and R. Renault.—On the taxonomic importance of the petiole, by M. Louis Petit.—On the reproductive organs of vegetable hybrids, by M. Léon Guignard.—On the relations of geodesy and geology: a reply to the observations of M. Faye, by M. A. de Lapparent.

BERLIN

Meteorological Society, October 5.—Dr. Brix, in the name of the Telegraph Administration, handed over to the Society a paper containing the results of observations respecting earth-currents instituted through the medium of German telegraph lines, and giving a brief history of these investigations.—Dr. Assmann spoke of the thunderstorms of the summer of 1886.

Physical Society, October 22.—Prof. von Helmholtz in the chair.—Prof. Börnstein communicated the results of his investigations into the thunderstorms of July 1884. The days from July 13 to 17 were very prolific in thunderstorms, and respecting them the speaker had collected and elaborated observations from more than 200 stations in Germany. For twenty-four separate thunderstorms, drawings were made of the "isobronts," isobars, and isothermals, from which it appeared that a fall in the barometer always preceded the outburst of the storm; that with the occurrence of the sinking of the barometer the atmospheric pressure rose very steeply and then relapsed gradually to its former level; and that the temperature, which was very high before the storm, declined rapidly with the outbreak of the storm. Local observations had formerly led to the same result. The "isobronts," or the lines uniting the places where the first peal of thunder was simultaneously heard, had in general a north-south direction. The "isobronts" made the passage from west to east with an average swiftness of from 38 to 39 kilometres an hour. The "isobronts" were attracted by the mountains, so that the part in whose west-east direction a mountain was situated approached it sooner, and, after the passage of the "isobront," delayed there longer than did the remaining part. Rivers retarded the progress of thunderstorms, and small thunderstorms often terminated at large rivers without crossing them. This relation of thunderstorms to mountains and rivers might be explained on the assumption that the storms were caused by ascending air-currents. When such an ascending air-current approached a mountain, then the mountain hindered the horizontal air from flowing in at the anterior side of the ascending current. The air flowing in at the posterior side, on the other hand, thereby obtained the preponderance, and urged the phenomenon with all the greater force to the mountain. The reverse occurred after the thunderstorm had surmounted the mountain. The horizontal currents in front then obtained the preponderance, and delayed the progress of the storm. The influence of the rivers found its explanation in the fact that the air above the water was considerably cooler than the air above the land, whereby a descending air-current was continuously maintained, operating in opposition to the ascending current of the thunderstorm, to the possible degree even of annulling it. The speaker had been able artificially to produce an imitation of all these processes by causing, in accordance with the directions of Dr. Vettin, visible currents to ascend in a glass box filled with tobacco smoke, by means of local depressions of temperature, by setting these currents in constant motion, and making them strike against obstructions (corresponding with the mountains), as also on descending currents which were likewise artificially created. In the discussion which followed the above address, Dr. Vettin laid stress on the fact that precisely at the moment when the barometer mounted steeply from its lowest position, the thunder followed the lightning most rapidly, and discussed how, in accordance with his conception of the nature of thunderstorms, by the curving round of the ascending air-current, a whirling movement round a horizontal axis came into shape, whereby, as determined by its situation and its extent, were produced thunderstorms, sleet, and hail.—Prof. von Helmholtz described the formation of a thunderstorm observed by him in Rigi-Kaltbad. From a free point of prospect, allowing a survey of the plain as far as the Jura, he observed how the lower warm and moist layer of air was distinguished by a sharp horizontal boundary of somewhat long strips of cloud from the upper dry and cooler air. The cloud-masses resembling the stripe-shaped cirri diffused themselves and formed a coherent level boundary-layer between the two air-masses. He next noticed, at different spots, balls of cloud arise above the boundary-layer, evidently as the effects of ascending air-currents. The different cloud-heaps then rose higher and grew into larger cloud-masses within which different electric sparks leapt from one spot to another. It was only subsequently that he saw the lightning fly downward to the earth. At last a heavy rain rendered the lower air-mass, bounded by the horizontal cloud-basis occupying a position nearly at a level with the height of the stand-point, which had hitherto been clear, opaque. The phenomenon had developed itself under weather in which the wind was at rest, and could be followed very precisely into its details.—Prof. Schwalbe reported on an investigation of Herr Meissner, who, in the Strasburg Laboratory, had determined the warmth effect on the wetting of powdery bodies. In the way of powder were used amorphous silicic acid, glass, emery, carbon; as fluids, distilled water, benzol, and amyl alcohol. In all cases an increase of temperature was observed.

BOOKS AND PAMPHLETS RECEIVED

La France en Indo-Chine: Bouinais and Paulus (Challamel, Paris).—Zeitschrift für Wissenschaftliche Zoologie, October 1886 (Engelmann, Leipzig).—Huddersfield Technical School Calendar for 1886-87 (Broadbent, Huddersfield).—Student's Hand-Book of Historical Geology: A. J. Jukes-Browne (Bell and Sons).—Units and Physical Constants, 2nd edition: J. D. Everett (Macmillan and Co.).—Principles and Practice of Canal and River Engineering, 3rd edition: D. Stevenson (Black, Edinburgh).—Monthly Weather Report, June 1886.—Quarterly Weather Report, January to March 1886.—Report of the United States Commission of Fish and Fisheries, Part 11, for 1883 (Washington).—Phantasms of the Living, 2 vols.: Gurney, Myers, and Podmore (Trübner and Co.).—Den Norske Nordhaus Expedition, 1876-78, XV. Zoologi; Crustacea, II.: G. O. Sars (Grondahl, Christiania).—Bulletin of the U.S. National Museum, No. 30: J. B. Marcou (Washington).—Proceedings of the Society for Psychical Research, October (Trübner and Co.).—Scientific Prevention of Consumption: G. W. Hambleton (Churchill).

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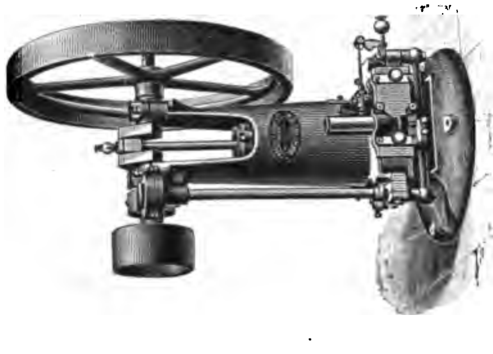

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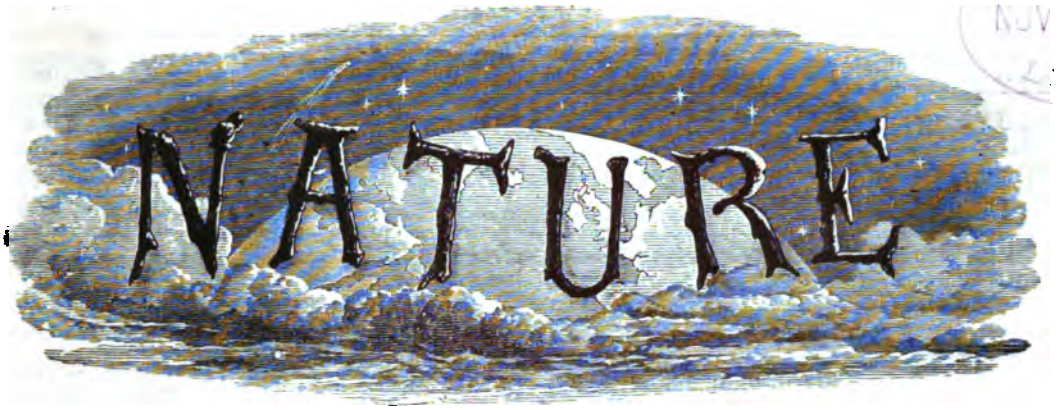

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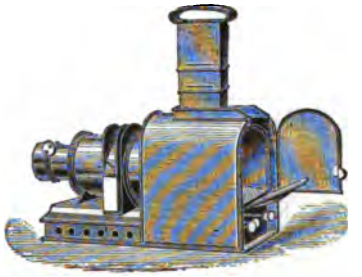

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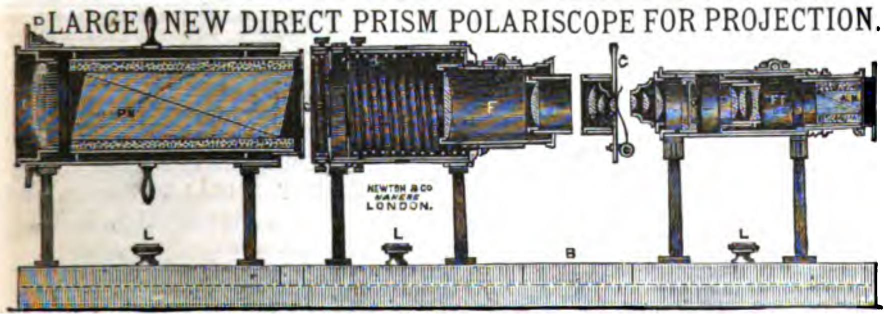

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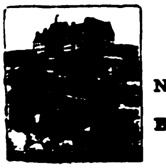

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THURSDAY, NOVEMBER 11, 1886

LETTERS AND JOURNAL OF W. STANLEY JEVONS

Letters and Journal of W. Stanley Jevons. Edited by His Wife. (London: Macmillan and Co., 1886.)

A STRIKING but sad book is this autobiography; for though "written to give the best idea of the character of the man in the various relations of life more than to recount scientific work," it is practically an autobiography: there is scarcely a critical remark upon his thoughts or conduct in it.

The family for many generations had been settled in Staffordshire. The grandfather came to Liverpool, and commenced business as an iron-merchant there, and his son Thomas, a man of ability in many ways, joined him in it. This was the father of William Stanley Jevons, who had, moreover, the almost invariable precedent of a clever man (*pace* Mr. F. Galton), viz. a clever mother, whom, however, he had the misfortune to lose at ten years old. She was the daughter of William Roscoe, author of the "Life of Lorenzo de Medici" and "Leo the Tenth." Another misfortune, from which, however, he learnt the value of money in a practical sense, befell him at the age of thirteen, when the firm of Jevons and Sons failed; and his grandfather, who died in 1882 at the advanced age of ninety-one, came to live with them.

A characteristic very marked, and to a marvellous extent affecting his whole subsequent life, was a bashfulness or "natural timidity of character which," his father wrote him, "is the worst, or perhaps I may say the only, weakness you have." This led to self-depreciation, and at school the French master complained that he was far too quiet and made no noise, and did not read above his breath. Shrinking from his companions and their fun, however, he early acquired the habit of directing his attention and mental powers at his will, and nothing tried his naturally passionate temper more than to be compelled to leave the pursuit of the moment while still engrossed in it. Reports of him as a scholar naturally kept continually improving, and, though laboriousness is throughout his characteristic, his sister writes in her diary that she saw in Stanley at the age of fourteen the dawnings of a great mind.

Botany and chemistry, in both of which he subsequently took honours, were the two sciences which attracted him first. The former was begun under the loving eye of his mother: the latter was the first that he took up at University College School, and "followed fiercer and fiercer till he gained the University gold medal."

He had decided at seventeen to go into a chemical manufactory at Liverpool, in order to remain near home; but before he had ended his last term of study at the University his wishes and plans were all upset by Profs. Williamson and Graham recommending him for the appointment of Assayer to the new Mint in Australia. He shrank from it as being too heavy a post for a youth of eighteen, and as going terribly against his wish to settle at home. But an income of 675*l.* a year was too good an offer to be refused. On June 29, 1854, not yet nineteen years old, he set sail for Sydney.

While at Sydney he attacked the Australian meteorology, and published his observations; more, as he explains, to show what phenomena had to be solved and what interesting connections of cause and effect might be suggested. Geology also, which he had commenced shortly before he left England, he there followed up. There he first suggested a collection of newspapers from all parts of the world as a curious exhibition; there also he heard of the death, after seven years of reviving prosperity in trade, of his father in November 1855. Though doing so well financially, he still cherished the feeling that he was losing time which he might put to better advantage. After four years he resigned his post, and on his return, *viâ* Callao, Panama, St. Thomas, Havanna, and several cities of the United States, he made his way up country past Minneapolis, to visit a brother who had gone out to settle there. Returning thence by way of Niagara and Montreal to New York, he landed at Liverpool, but soon went on to London and re-entered the University. He joined several senior classes in company with his younger brother, whose education he was then paying for. He had decided thenceforth to follow up political economy and mental philosophy.

His "Theory of Political Economy" was read as a paper but not "approved" by the British Association at Cambridge in 1862. It was published in 1871, and reached a second edition in 1879. Though it attracted the attention of some eminent foreigners, it was coldly received in England—the free use of mathematical symbols placing it above the heads of those practically engaged in commercial pursuits. In 1875, at the British Association meeting at Bristol, he read another well-known paper on the connection between sunspots and the price of corn—bad crops of the latter, we need hardly add, being followed by a high price and bad trade—and though he spoke at first very doubtfully of his theory, yet up to the time of his death, in 1882, he believed that a great revival of trade would take place almost immediately, to be followed by seven years of unprecedented prosperity, and he had speculated accordingly. Gold, however, alas, seems a more important factor than sunspots.

A more famous paper still was his "Coal Question," published in 1865. It was a question in which the whole nation took an interest, and it supplied a text for one of Mr. Gladstone's economical budgets. Accordingly it was discussed in every paper, political, economical, or social, and is perhaps better known now than any of his other writings.

His earlier writings had brought him in very little, and in 1863 he had accepted the not very lucrative post of tutor at Queen's College, Manchester. In 1866 he was appointed Professor of Logic and Mental and Moral Philosophy, and Cobden Professor of Political Economy, at 300*l.* a year. A thorough teacher, he was much liked by his pupils, never tiring of making them understand, and watching their careers in after life.

In December 1867 he married the daughter of Mr. J. E. Taylor, founder and proprietor of the *Manchester Guardian*. To her we are indebted for this well-arranged selection of letters.

In 1864 he published his first work on "Pure Logic," chiefly founded upon Prof. Boole's system. In 1865 he invented a logical machine or abacus which he describes as working in a few moments any logical problems involving no more than four distinct terms or things. It was like a small piano, three feet high, with twenty-one keys. A second book upon logic was published in 1869, just after this had been made to work correctly, entitled "The Substitution of Similars," containing a sketch of the fundamental doctrine of his great work, "The Principles of Science," which was not published in full till 1874, but reached a second edition in one volume in 1877.

In 1870 his "Elementary Lessons in Logic" appeared in Macmillan's series of science class-books, followed in 1876 by the "Primer of Logic," one of the same publishers' more elementary series; and in 1880 "Studies in Deductive Logic" for students desiring a more thorough course of logical training.

In 1868 he had prepared three articles attacking J. S. Mill's system of logic. They were declined at first, but three years afterwards, soon after the death of Mr. Mill, they were accepted by the *Contemporary Review*. It is curious to see two such mighty champions of such a learned science referring their differences to an uneducated public and to their instinctive logic!

Though sorry on many accounts to leave Manchester, his heart had never left London and its University, to which he returned in 1876 as Professor of Political Economy. In that year he boldly read a paper laying it down that the United Kingdom Alliance was the worst existing obstacle to temperance reform in the kingdom—driving the enemy to a man into fierce opposition.

His first illness through over-work had occurred in 1869, and from that time his letters in large proportion are from various places — Norway was his favourite resort—to which he had been driven to regain strength. Trip after trip was taken, but with no permanent effect. As soon as he returned he again overwhelmed himself with work, involving too great tension of the brain. The labour especially of taking his class when out of sorts was a "painful" labour to him. To relieve himself from this he resigned his Professorship in 1880, and in 1882, after two years more of work at home, but still at high pressure, a plunge into the sea was too sudden a chill for his enfeebled frame, and insensibility and death were the sad result, at the prime age of forty-six.

One cannot help sorrowfully noting how his childish bashfulness was the cause of his early death. It led to unsociability and abstinence from recreation. Instead of rejoicing in his strength, he shunned his companions, and persuaded himself, moreover, that it was his duty to do so, though he bitterly regrets it afterwards, one result being an inability to speak in public and communicate his ideas as he would wish. The ardent cultivation of his many talents, again, increased a feeling of superiority, yet often left him low-spirited. In some it might have brought carelessness and improvidence, but in Jevons it was attended by a feeling of responsibility almost religious. At twenty-three he threw up his easy and lucrative post at the Sydney Mint in obedience to this feeling, and, later on, he resigned one laborious duty only to buckle to another, and under such labour his life was quenched.

GENERAL PATHOLOGY

An Introduction to General Pathology. By J. B. Sutton, F.R.C.S. (London: J. and A. Churchill, 1886.)

UNTIL recently, pathologists have confined their attention to studying the processes of disease in human beings, and but little effort has been made to take advantage of the vast field of material presented by the animals which die in the Gardens of the Zoological Society. Since 1878 the author has systematically examined the bodies of 12,000 animals and of over 800 still-born and immature fœtuses; and from this vast stock of material he has, for the purposes of the present work, selected, from all parts of the animal kingdom, striking examples which illustrate the main pathological and physiological processes of life. The same principles govern both, and processes which in one group of animals are the cause of disease, in another, owing to anatomical differences, habits of life, and surroundings, have no such influence. Moreover, pathological defects are frequently inherited, and become looked upon as racial peculiarities. Thus the horns of the Ungulata, the curved canines of the Babiroussa, the atrophied right ovary and right carotid artery in many birds, the large third with the small second and fourth metarcarpals of the horse, are now persistent, but were probably originally accidental and pathological.

The degree of development of the muscular tissue of the gizzard of a bird is dependent upon the nature of its food. The herring-gull of the Shetland Islands changes its food twice every year—in the summer living on grain, when its gizzard is of the granivorous type, and in the winter on fish, when the gizzard reverts to the carnivorous condition. The same variations have been artificially produced by varying the food of sea-gulls, pigeons, ravens, and owls. While excessive function is the great cause of hypertrophy of organs, deficient usage is the determining factor in the abnormal overgrowths of hair, nails, beaks, and teeth. Rodents in captivity frequently require their teeth to be artificially shortened in order to avert the fatal effects of excessive overgrowth.

Monkeys, when in confinement, frequently die with symptoms of more or less complete paraplegia, which has recently been shown to be due to an overgrowth (frequently rickety) of the vertebræ near the intervertebral lamellæ. This gradual compression of the cord also occurs in tigers, lambs, bears, and others. These facts observed in animals throw light upon the agonising pains of mollities ostium, which are doubtless in like manner due to compression of the cord and nerves, which is permitted by the softening of the bones which the disease causes.

Metschnikoff's definition that inflammation is a struggle between irritant bodies and white blood-corpuscles is adopted. Illustrations are given showing the white corpuscles surrounding and digesting micro-organisms and other foreign bodies, or dying in the attempt to do so. When the tails and gills of larval batrachians are being absorbed, numerous amœboid cells can be seen containing fragments of nerve-fibres and muscle.

Our present knowledge of the nervous system quite fails to offer any explanation of the experiment which the author performed by transposing the median and ulnar nerves in a cat. The two nerves were divided about the middle, and the distal end of the median was united to the proximal end of the ulnar, and *vice versâ*. Union occurred: sensation and motion returned in six weeks. The cat regained complete use of the limb, and did not appear to suffer any inconvenience. It would be of great interest to repeat the experiment by attaching the proximal end of the median to the distal end of the ulnar, but to prevent the other ends from uniting. Would the result be an unimpaired function of the median nerve, transmitted from the portions of the brain and cord formerly associated with the function of the ulnar?

The classification of the cysts and neoplasms of the animal kingdom are illustrated by many typical examples. The teratomata are fully discussed, and it is shown that they almost invariably occur only in spots where the epi-, meso-, and hypo-blastic layers have been temporarily but directly in continuity with each other. Thus præsacral tumours are associated with the obsolete neurenteric canal; pituitary tumours with the canalis craniopharyngeus (a canal through the floor of the basisphenoid), lingual dermoid cysts with the ductus thyreoglossus (a canal running from the basihyoid to the foramen cæcum of the tongue), and ovarian dermoid cysts with the obsolete Müllerian and Wolffian ducts. The predisposition of obsolete ducts to disease has for some time been recognised, but the close relationships of teratomata to such ducts is a more recent piece of work.

The novel illustrations of the leading pathological processes make the work one of extreme interest, and we heartily congratulate the author on the good use to which he has turned his exceptional opportunities.

PLANE GEOMETRY

The Elements of Plane Geometry. Part II. (corresponding to Euclid, Books III., IV., V., VI.). (London: Swan Sonnenschein, 1886.)

THIS book contains a revised edition of Books III., IV., V., of the "Syllabus of Plane Geometry" drawn up by the Association for the Improvement of Geometrical Teaching, with demonstrations of the propositions, and an excellent, though limited, collection of suitable exercises. If nothing else than these two parts had been the outcome of the movement first set on foot in our columns, the Association would have amply justified its formation. Much difference of opinion has prevailed as to the desirability or expediency of the Association producing such a work as this. The late Mr. Merrifield for some years strenuously opposed any such proceeding, but at the annual meeting of 1881 he expressed himself as "now satisfied from the experience which he had had in dealing with the examining bodies that they would not get their work really adopted by the public until they had a text-book. Everywhere he was met with the impossibility of wading through a dry Syllabus. Nobody who was not thoroughly versed in mathematics could judge whether there was any real possibility of teaching from the Syllabus at all." Circumstances appear to have compelled the Association at last to take the field with demonstrations put forward by a selected committee of its members: a principal reason being that the Association was bound to help teachers. The plan of teaching the Syllabus without giving written proofs was found to succeed so long as the teaching was confined to the earlier parts of the subject, but when the later books were reached it was found necessary to give formal written proofs for subsequent reference (Report, 1881, p. 30).

Some teachers who wish for the more copious introduction of modern ideas and methods into the very elements may not consider the work of the Association as satisfactory as could be wished, and may think there is very little of the influence of the aforesaid modern ideas in the Syllabus, yet even such admit, and express satisfaction in making the admission, that "the use of the Syllabus has spread pretty widely, and it is to be hoped that it will continue to do so" (Prof. Henrici, British Association address, NATURE, vol. xxviii. p. 500). It is to be borne in mind that the Professor hardly gave the Syllabus a fair trial, though he says that when it appeared "I resolved to give it a thorough trial, and took the best means in my power to form an opinion on its merits by introducing it into one of my classes. The fact that it did not quite satisfy me, and that I gave up its use again, does not of course prove that it fails also for use in schools, for which it was originally intended."

These students had, we assume, to take down in writing the Professor's proofs, and it is not as agreeable work to "grind up" manuscript as it is to read a printed page; then there would be by-gone remains of the old text-book haunting the students' brains, want of familiarity with the Syllabus possibly on the teacher's part, and finally shortness of time over which the trial extended. At this point we may cite some remarks by the late Dr. Todhunter, which make as much for Syllabus upholders as for Euclidians. "It will be hard to secure that pupils shall be selected of equal power, and be trained with equal assiduity; and then if our teacher is to try various methods he is liable, since he knows that a controversy is now existing as to the result, to deviate from impartiality in his treatment of the rival methods. Moreover, there may naturally arise some disagreement as to the means to be used for testing the value of the results, and as to the accurate application of the principle which may be finally adopted for this end" ("Conflict of Studies," &c., p. 156). A fair trial would be to take two classes of students of as nearly as possible equal mental calibre, and with equal want of acquaintance with geometry, and to take each through the respective courses for the same time, and to take care that each teacher should be equally skilled and acquainted with his author and equally enthusiastic, for, as our essayist just cited writes, "if the teacher is only languid without being positively hostile, his real sentiments are soon discovered: hypocrisy has but a slender chance of deceiving school-boys" (p. 164). But such a fancy is Utopian; the hope of the Association at first lies in such far-away parts as the Cape and India, where its work is being taken up by enthusiastic and able teachers.

We have read the proofs, and believe them to be thoroughly accurate; there is also a careful avoidance of all looseness of language. Dr. Todhunter's "deliberate judgment" was that "our ordinary students would suffer very considerably if instead of the well-reasoned system of Euclid any of the more popular but less rigid manuals were allowed to be taken as a substitute" (p. 168). So fearful was he of looseness or slipshoddiness that he more than once returns to this matter, and upon this very point of an Association text-book writes as follows:—"There are various considerations which seem to me to indicate that if a change be made it will not be in the direction of *greater rigour*" (p. 172). He owns himself once to have been in favour of *hypothetical* constructions, but that he had subsequently seen reason to alter his opinions: in many places in his essay he shows that he has not renounced *hypothetical* statements. His idea of an Associationist seems to have been that he is a being who tries to evade the difficulty of passing a pupil in geometry by asking for a less stringent text-book than that of Euclid.

It is vain to wish for the verdict of such able critics as De Morgan and Todhunter on the work before us, but we feel sure that the former would not have written concerning it "Non est geometria," nor the latter have found it wanting in Euclidian rigour.

As to this matter of a different order from Euclid's sequence we cite with cordial approval the following remarks of a writer in our columns (vol. xxxiv. p. 50):—"We believe that those who have most carefully considered the question of a rival order of sequence of geometrical propositions would agree that the best order in a logical arrangement does not seriously *conflict* with Euclid's order, except by simplifying it. Rather, by bringing the proofs of each proposition nearer to the fundamental axioms and definitions than Euclid does, it renders less assumption of previous propositions necessary for the proof of any given proposition. It stretches the chain of argument straight instead of carrying it round one or many unnecessary pegs."

The influence which the Syllabus has had upon modern editions of Euclid is patent to any reader of the works in question. And now, little book, that the Association has at the end of days sent forth on to (it may be) tempestuous seas, we wish thee *bon voyage!*

OUR BOOK SHELF

American Journal of Mathematics. Vol. viii. No. 4. (Baltimore, August 1886.)

THE number opens with a memoir, by M. Poincaré, "Sur les Fonctions Abéliennes." The author gives here a *résumé*, with additional details, of a demonstration and generalisation of two of Weierstrass's theorems, which he had previously published in the *Proceedings* of the Mathematical Society of France (tome xii. p. 124). He then extends a theorem of Abel's from plane curves to surfaces, and refers, for fuller details, to a crowned memoir of M. Halphen's, "Sur les Courbes gauches algébriques." He next discusses some properties of "fonctions intermédiaires," using the term in the sense given by MM. Briot and Bouquet. This memoir occupies fifty-four pages. The second paper, on "A Generalised Theory of the Combination of Observations so as to obtain the best Result" (24 pp.), is by the editor, Prof. Newcomb. A very valuable article, with important practical applications. The final article (22 pp.), "On Symbolic Finite Solutions and Solutions of Definite Integrals of the Equation—

$$\frac{d^n y}{dx} = x^m y,\text{"}$$

is by Mr. J. C. Fields. It discusses finite solutions analogous to the symbolic solutions of Riccati's equation.

A Sequel to the First Six Books of the Elements of Euclid; containing an Easy Introduction to Modern Geometry (with numerous Examples). By John Casey, LL.D., F.R.S. (Dublin: Hodges, 1886.)

THIS is the fourth edition of a book which has been received with warm approval by English and Continental geometers. The first eight sections present no notable changes from the corresponding sections in the last edition. In our previous notice (NATURE, vol. xxix. p. 571) we remarked that the author was "not so well up in the literature of the modern circles as he might be." This reproach is quite removed in the present edition. Indeed in this direction the author has himself now done excellent yeoman's service. The "supplementary chapter" of fifty-eight pages gives an admirable account of this modern branch in six sections. The first section states and illustrates the theory of isogonal and isotomic points, and of anti-parallel and symmedian lines. The second discusses "two figures directly similar" in homothetic figures. The third section is headed "Lemoine's and Tucker's circles." The fourth discusses the "general theory of a system of three similar figures." The fifth gives "special applications of the theory of figures directly similar," more particularly with reference to Brocard's circle and triangles. In the sixth section on the "theory of harmonic polygons," the author, starting from Mr. Tucker's extension of the Brocard properties to the harmonic quadrilateral, and Prof. Neuberg's continuation of the same, gives his own beautiful generalisations to the harmonic hexagon and other allied polygons. This latter extension has been made the subject of a communication by MM. Tarry and Neuberg to the French Association meeting at Nancy in August of the present year. The paper, which is not expected to be published until April 1887, contains a complete generalisation of points of Lemoine and Brocard, and the modern circles cited above for polygons and polyhedra.

The success of the "Sequel" is due to the fact that the author and the subject are exactly suited to each other: the union is a most harmonious one, and the result is a work indispensable to all lovers of geometry.

Geometrical Drawing for Army Candidates. By H. T. Lilley, M.A. Pp. x., 54. (London: Cassell and Co., 1886.)

IN a short introduction to this little work the author gives some useful advice to those beginning practical geometry, and rightly lays stress on the proper method of handling instruments, and on a good style of working.

The book contains altogether 300 problems in plane constructive geometry; they are nearly all straightforward and easy, but 180 of them are specially indicated as forming, according to the author's experience, a suitable first course for the majority of students.

The problems are conveniently grouped together, and hints are given in aid of the solution of typical ones, and of those presenting extra difficulty. Beginning with the construction of scales, we have the usual series on polygons, proportionals, equivalent areas, and, in conclusion, several cases of circles touching other circles or given lines.

As a book of examples this collection seems likely to prove useful in class-teaching. But in order to insure sound instruction, much that is not contained herein will have to be provided for the student. Thus in the notes to the problems before us no reasons are given or indicated for the various steps in the constructions, and there is no distinction drawn between those methods of construction which are exact, and those which do not admit of proof.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Enormous Loss from Ox-Warble

I HAVE read Mr. John Walker's remarks on "warbles." This is one of the many important subjects to which Miss Eleanor Ormerod has lately drawn attention. I can readily believe that there is a loss of two to three millions to the country through the ravages of this fly, but such statements, it must be remembered, should be qualified by the thought that it might cost two or three millions to protect all the cattle of this country against such attacks. The labour would be great, the vigilance would entail higher-classed stock-men, in almost all cases with higher wages, for you cannot get our labourers, dairy-men, and bailiffs even, to attend to such matters without great difficulty. The loss does not, I think, fall upon farmers, unless it is from the irritation to the cattle when they hear the buzz of the fly meditating her attack.

As to the damage to the hide, I never, in my experience, heard a butcher or dealer make warbles in the hide a pretext for offering one shilling less for a bullock. They take no notice of them at all; and, if the maggots injure the hide, this is a matter for fell-mongers and tanners, rather than for farmers. This is one of the cries emanating from the scientific friends of agriculture which it is well to listen to. It will probably gain the ear of only a select circle of agriculturists, because, to use a very homely phrase, "the game is scarcely worth the candle." Animals pass through the market too rapidly, and the prices asked and given are so approximate only to the absolute value, that a few warbles in the skin do not in the least influence the selling price. Still, anything which can be shown to influence the comfort of live stock or the value of their products must be considered as worth attention.

JOHN WRIGHTSON

College of Agriculture, Downton, Salisbury, October 31

"Lung Sick"

MR. H. RIDER HAGGARD, in his excellent novel, "King Solomon's Mines," has the following passage. He is speaking of Zulu oxen, and says:—

"As for 'lung sick,' which is a dreadful form of pneumonia very prevalent in this country, they had all been inoculated against it. This is done by cutting a slit in the tail of an ox, and binding in a piece of the diseased lung of an animal which has died of the sickness. The result is that the ox sickens, takes the disease in a mild form, which causes its tail to drop off, as a rule about a foot from the root, and becomes proof against future attacks."

Presumably this account is *bonâ fide*. It will be gratifying to me, then, if any of your correspondents will kindly explain how it is that the virus, which has not been weakened by cultivation, produces the disease in a mild rather than in a virulent form.

E. J. DUNGATE

6, Marchmont Road, Edinburgh, November 1

The Beetle in Motion

WITH reference to Prof. Lloyd Morgan's letter in last week's NATURE (p. 7), the following passage, which occurs in an interesting chapter on "Motions of Insects" in Kirby and Spence's "Entomology," may be quoted:—

"In walking and running, the hexapods, like the larvæ that have perfect legs, move the anterior and posterior leg of one side and the intermediate of the other alternately."

This passage is in complete accord with the observations of your correspondent.

C. J. G.

November 9

Meteors

YESTERDAY (November 2), about 8.8 p.m., I chanced to see here a meteor that, I think, deserves record, especially if my report of its position in the sky can be compared with that of some one who observed it at another place.

Returning from Oxford, I was about half a mile east of Combe Church, on the lofty plat that is the remnant of Combe Common. "Stepping westward," I was startled by a sudden splendour, flooding with light the moonlit heaven. This splendour was above me and before me; it was a little on my left. A large meteor was rapidly descending, at an angle of 60° or 70°. Not much east of it shone the half-orbed moon; but little west of it stretched the eastern branch of the Milky Way's western termination. When it had traversed about three-fourths of the distance between its apparent starting-point and the undulating ground beneath, it swelled out for a moment grandly, and, before it burst, displayed a globe at least as big as the sun, and of about the same hue, though not of dazzling lustre. After it had vanished, its track was marked for a second or two by a brilliant trail, which, in the light of the neighbouring moon, sparkled with all the tints of the rainbow, and resembled a gorgeous shower of precious stones.

JOHN HOSKYNS-ABRAHALL

Combe Vicarage, near Woodstock, November 3

I HAVE read Mr. Murphy's letter (NATURE, November 4, p. 8). At the same time as Mr. Murphy saw a large meteor (October 31, 8.25 p.m.) I also saw an immense one coming from the same portion of the sky, and travelling west. It disappeared behind a cloud. There was a loud rushing noise.

E. PARRY

Dinorwic Quarries, Llanberis, North Wales

INFLUENCE OF WIND ON BAROMETRIC READINGS

I AM glad to see (NATURE, vol. xxxiv. p. 461) that the Scottish Meteorological Society recognises the importance of the effect of wind upon the barometer. I assume that the gradient, the density, and all other sources of error had been fully corrected for before concluding the existence of the large effect attributed to the wind on Ben Nevis.

There certainly is a purely local and dynamic effect of the wind on the barometer due to the exposure, and for which there must be found some method of correction or elimination before we can proceed much farther in barometry: this effect has been independently reasoned out by G. K. Gilbert ("A New Method, &c.," 1883), and has been discussed by Prof. H. A. Hazen (*Annual Report*, C.S.O., 1882, p. 897), and by Mr. Clayton and others in recent numbers of *Science*, but its existence was long since demonstrated by Sir Henry James (*Transactions* Roy. Soc. Edinburgh, vol. xx., 1853), whose memoir seems to have been quite lost sight of by meteorologists.

The suction of wind on tubes, cowls, and chimneys was investigated by Ewbank (*Journal* of the Franklin Institute, 1842), Wyman (*Proceedings* of the American Academy, Boston, 1848), Fletcher (*B.A.S. Reports*, 1867 and 1869), Magius (Copenhagen, 1875?), Holten (Copenhagen, *Oversigt Vidensk-Selskabs*, 1877), and was used by Hagemann as the basis of his anemometer; it was Hagemann's memoir (Copenhagen, 1876, translation will appear in *Van Nostrand's Magazine*, Dec. 1886) that suggested a method of determining and correcting for the amount of this important effect, whose existence had long been known to me. This method is sketched out in the *Annual Report* of the Chief Signal Officer, U.S.A., 1882, p. 99, where I state that a close determination simultaneously of both dynamic wind-pressure and static air-pressure is probably attainable by exposing above the roof, side by side, a Pitot tube facing toward the wind and a vertical tube over which the wind blows. Close the lower ends of these tubes and place within each an aneroid barometer, and the latter will record respectively the static pressure plus the effect of the wind-velocity and the static pressure minus the wind's effect. A stop-cock, cutting off at will communication between the aneroids and the exposed mouths of the tubes, allows one to catch the influence of any gust and read the pressure at leisure.

The theoretical problem of the precise mechanical action of these tubes, especially that which Hagemann calls a Magius tube, *i.e.* one across which the wind blows at right angles, will, I hope, prove attractive to the mathematical physicists of England. Some interesting experimental work by Robinson will be found in *Van Nostrand's Magazine*, vol. xviii., 1878, p. 255, and xxxv., 1886, p. 89. A small closed room with only a chimney flue opened, such as usually obtains at the mountain stations of meteorologists, is virtually a Magius tube, and the barometer within must, under favourable conditions, show a depression depending on the so-called suction or draft up chimney. The direction of the wind combines with the structure of the building and the aspect of the various doors and windows to modify the influence of the force of the wind; the sluggishness due to the close cisterns, and the pumping due to the inertia of the liquid of ordinary mercurial barometers, further complicate the phenomena of suction during gusty winds, so that a simple general rule for correcting the observed barometric readings becomes impracticable, but the use of aneroids within closed Pitot or other tubes, with air-tight stop-cocks as above, simplifies the wind's action, and allows of its measurement at definite moments.

The distribution of pressure over the face of a large building fronting the wind, and in some part of which is the window of the room containing the barometer, is approximately known from Curtis's and Burton's measurements for a thin flat plate.

The location of each station with respect to mountains or other orographic features has also an influence on the pressure, which will still remain to be investigated; thus, on the leeward side there is a diminution, and on the windward side an increase of pressure, but this may be generally inappreciable.

It may also be mentioned in this connection that in delicate barometric measurements, such as those made by the International Bureau of Weights and Measures, it is important to prevent even the slightest currents from blowing across the open end of the siphon tube.

The suction effect of wind blowing over chimneys surmounted by cowls of different shapes was under investigation from 1878 to 1881 by a special committee of the Sanitary Institute, but, so far as I can learn, their experiments were never completed. Lord Rayleigh also read a short paper on the same subject at the meeting of the British Association in 1882, but as I do not know of its publication, I take this opportunity to express the hope that he will give meteorologists both a theoretical and experimental exposition of the action of the Pitot, the Magius, and the reversed Pitot tubes, and a suggestion as to the best method of determining, by means of stationary apparatus, the static pressure within a mass of moving air. CLEVELAND ABBE

Washington, October 23

M. PASTEUR'S TREATMENT OF RABIES

AT the meeting of the Paris Academy of Sciences on November 2, M. Pasteur submitted a further communication on the results hitherto obtained from his method of treating hydrophobia by inoculation, which has now been in operation for a twelvemonth. The paper is divided into three parts, the first giving the statistical details brought down to the present date, the second describing certain modifications in his method as originally applied, the third giving the results of fresh experiments on animals. Up to October 31 as many as 2496 persons were inoculated at his Paris establishment, and at first the treatment was uniform for all alike, whatever their age, sex, or other varying conditions. Of the total number 1726 were from France and Algeria, 191 from Russia, 165 from Italy, 107 from Spain, 80 from England, 57 from Belgium, 52 from Austria, 22 from Roumania, 18 from the United States, 14 from Holland, the rest from various other parts of Europe, besides 3 from Brazil and 2 from British India. Of 1700 French patients, apart from 2 who arrived too late, 10 only succumbed, whereas of the small minority not treated at the laboratory as many as 17 died in the same period in the rest of France, while for the last five years the average yearly mortality from hydrophobia was 11 in the Paris hospitals alone. Last year it rose to 21, but since November 1885, when the new system was introduced, 2 only died, and these had not been inoculated, besides a third who had been imperfectly treated. Most of those who perished were children bitten in the face and subjected to the simple treatment, which experience now shows to be insufficient in such cases.

A first lesson on the necessity of stronger doses was taught by the 19 Russians bitten by a mad wolf, one of whom died while under treatment, and two others shortly after. In consequence of these deaths the 16 survivors were subjected to a second and third treatment with the strongest and freshest virus from the spine of the rabbit of 4, 3, and 2 days' standing, whereas, for the milder treatment, virus from 14 to 5 days' old had alone been used. To these repeated treatments should most probably be attributed the recovery of these Russians, who are reported to be all still in excellent health.

Encouraged by these results and by the fresh experiments described further on, M. Pasteur modified his treatment, making it at once more rapid and more active for all cases, and even still more energetic for bites on the face, or for deep and numerous lacerations of exposed parts of the body. In such cases the inoculations are now hastened, in order to arrive more promptly at the freshest virus. Thus, on the first day, virus of 12, 10, and 8 days will be used at 11, 4, and 9 o'clock; on the second day that of 6, 4, and 2 days, at the same hours; on the third, virus 1 day old. Then the treatment is repeated: the fourth day with virus 8, 6, and 4 days old; the fifth with that of 3 and 2 days; the sixth with that of 1 day; the seventh with virus of 4 days; the eighth with that of 3; the ninth that of 2; the tenth with that of 1 day.

If the bites are not healed, or the patients arrive somewhat late, the same treatment may be renewed at intervals of two or a few days for four or five weeks, which are the critical periods for children bitten in the face. This system of vaccination has been in operation for the last two months, hitherto with excellent results, as shown by comparing the case of the six children who perished under the mild treatment, with that of ten others also seriously bitten last August, and subjected to the more energetic treatment, and all of whom were doing well on the first of this month. This new system requiring an increase of the staff, M. Pasteur and his assistant, Dr. Grancher, have been aided for some time past by Dr. Terrillon, Dr. Roux, Dr. Chantemesse, and Dr. Charrin.

With regard to the fresh experiments on dogs, an objection to the inoculation of human beings after being bitten might be raised on the ground that the immunity of animals treated before being bitten had not been sufficiently demonstrated after their undoubted infection by the virus. In reply to this objection M. Pasteur points to the immunity of dogs after trepanning and intra-cranial inoculation with the virus of ordinary street rabies. Trepanning is the surest method of infection, and its effects are constant. The first experiments on this point, dating from August 1885, had but partial success. They were resumed during the last few months, with certain modifications which produced the best results. The vaccination is begun the day after inoculation, and proceeded with rapidly, the series of prophylactic virus being all administered within twenty-four hours and even in a shorter period, and then repeated once or twice at intervals of two hours. The failure of Dr. Frisch, of Vienna, in experiments of this kind is due to the slow process of vaccination adopted by him. Success can be secured only by the rapid method here described. The immunity conferred under such conditions is the best proof of the excellence of this method.

REPORT ON THE CHARLESTON EARTHQUAKE[1]

THE earthquake of August 31, which, from the locality in which its greatest power was displayed, will generally be known as the "Charleston Earthquake," was, perhaps, the most notable disturbance occurring within the limits of the United States of which we have any knowledge. It is entitled to this rank both on account of the wide area over which it was distinctly felt, and of the magnitude of the disaster which it caused in the immediate vicinity of the point of maximum intensity.

The earthquake consisted of a series of seismic disturbances which began in slight but distinctly noticeable tremors occurring on August 27 and 28, at the town of Summerville, about twenty-five miles north-west of Charleston, South Carolina.

The shock of greatest violence occurred a little before ten o'clock on the night of Tuesday, August 31. It was followed by several of lesser magnitude on that night, and during the succeeding three or four weeks. The great shock began in the city of Charleston within a few seconds of 9.51 p.m., 75th meridian time. The duration of the vibratory motion of the earth at that point was probably about forty seconds; the motion at first being moderate, but increasing with great rapidity during the last ten or fifteen seconds.

All of the loss of life and property during the whole series of disturbances is to be attributed to this first shock. Five minutes later another occurred, and ten minutes later still another; the latter being of considerable violence, but neither alone would have done any damage. The same may be affirmed of the succeeding series of disturbances, which, with greatly diminished intensity and at increasing intervals of time, continued to maintain the conditions of alarm and terror into which the people of the afflicted locality were naturally thrown by the first disturbance. Although some injury to buildings resulted from these after shocks, it is tolerably certain that in all such cases displacement and fracture had taken place in the great shock; the lesser disturbances simply finishing what had then been nearly completed.

The origin of the disturbances, appears to have been somewhere below a point fifteen or twenty miles north-west of Charleston; that is, in the neighbourhood of the town of Summerville. A chart of provisional co-seismal lines drawn by Mr. Hayden of the Geological Survey, and published in *Science* for September 10, seems to locate this centre somewhat further north than the point indicated above. At the time of its construction, however, information from many points was lacking, and that which was at hand was admittedly doubtful in some degree.

Reference is made later to the iso-seismal chart which accompanies this Report, and which indicates that the origin was near the point referred to above. Strong proof of this is also furnished in the intensity and character of the disturbance as shown by the effects which were still visible when an examination was made a few days after the principal shock. The appearance of the brick piers upon which many houses in Summerville rest was such as to justify the conclusion that the principal component of the motion at that point was vertical, and it was evident that the destruction of buildings was much less than would have resulted from a horizontal movement equal to that which had taken place in Charleston and elsewhere in the neighbourhood.

Another fact of importance is that in the vicinity of Summerville the disturbances preceding that of August 31 took place, and here they have been most numerous and most persistent. Indeed, at the present writing, nearly a month after the first perceptible shock, they still occur at irregular intervals varying from a few hours to a few days. Only the most violent of these have been felt as far as Charleston.

Nearly all the movements in Summerville and vicinity have been accompanied by, and, indeed, generally preceded by, a low rumbling sound, lasting one or two seconds, and not unfrequently this sound, always unmistakable in its character, was neither accompanied nor followed by a perceptible movement. This was a common occurrence at Summerville and in the immediate vicinity, and it was found that among several observers there would be no agreement upon the direction from which the sound appeared to come.

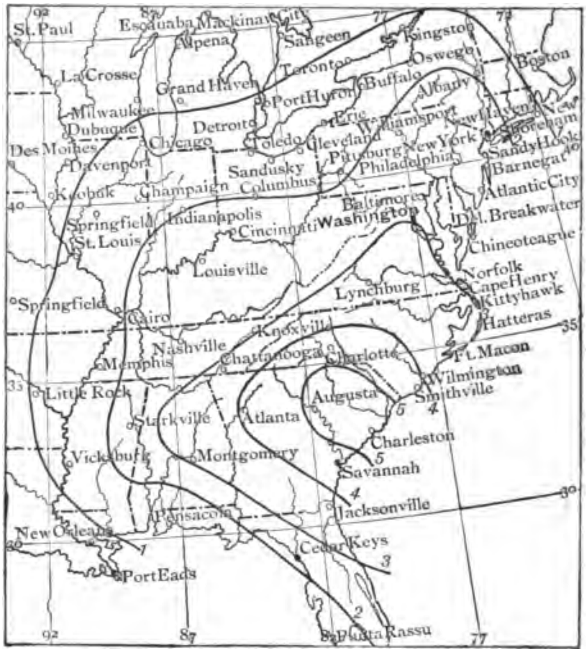

Chart of iso-seismal lines.

At a distance from ten to fifteen miles from Charleston in the direction of Summerville some of the most curious and interesting effects of the disturbance were to be seen. These were the "sand craters" and crevices, out of which extensive eruptions of sand and water had taken place on the night of August 31. The craters thus formed varied in size from an irregular oval, twenty-five feet long by fifteen feet wide, to shallow cones not over an inch in diameter and beautifully symmetrical in form. The area surrounding these openings was generally flooded with sand, often acres in extent, to a depth varying from a fraction of an inch to fifteen and eighteen inches. About the larger cavities the average depth was probably not less than six inches, and the area covered often an acre or more. The flow of sand was unquestionably only an incident to the outflowing of vast quantities of water, the greater part of which disappeared within a few hours after its appearance. The few crevices or "cracks" in the earth which were found were in character and origin similar to the "craters," being long and narrow openings, through which water with sand had been ejected.

It was difficult, in fact quite impossible, to obtain reliable information concerning the nature of this phenomenon at the moment of its occurrence. The locality in which it was principally exhibited is near a station on the South Carolina Railway, between Charleston and Summerville, known as "Ten-mile Hill." It is thinly populated, and almost entirely by negroes. Several persons who pretended to have been eye-witnesses of the outburst gave widely different testimony as to its character. According to one account, the water and sand from one of the "geysers" spouted to a height greater than that of a telegraph pole and continued to flow for four or five hours. Another, and apparently an equally credible witness, declared that the stream reached a height of six or eight feet, and that the flow continued four or five minutes. The latter statement is probably nearer the truth than the former.

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[1] By Prof. T. C. Mendenhall, Assistant. From the *Monthly Weather Review*, U.S. Signal Service, August 1886.

A few instances of sand eruptions were found in the city of Charleston, and a few also at Summerville, and at the latter place water continued to flow from one of the openings for several days after the first shock.

It is important to observe that in no case was it found that the water thus issuing from the earth was hot or noticeably above the temperature of water in shallow wells in the neighbourhood. Reports of boiling water having been thrown up were very numerous, but no evidence that the water was really hot appeared. The use of the word "boiling" doubtless grew out of the appearance of the water as it issued from the openings, and was probably used by eye-witnesses to describe this appearance with no reference whatever to temperature.

There were also reports of the appearance of blue flames in the neighbourhood of these eruptions, but no reliable testimony to their existence could be obtained. There was also a report that was circulated extensively through the medium of the press of the country that two or three showers of hot stones had fallen upon and near the office of the *Charleston News and Courier*. An examination of some of these shortly after they had fallen forced the conviction that the public was being made the victim of a practical joke.

In the city of Charleston about forty lives were lost. The greater number of casualties resulted from injuries sustained by persons who were either in the street at the time of the shock or who rushed out and were caught by the falling debris. No adequate description of the injury to property can be given in this place, and, indeed, the results of this earthquke have been so thoroughly considered in the public press that note is unnecessary.

While there was probably not a single house in the city which was not in some degree affected by the shock of August 31, there was naturally great diversity as to the extent of the damage in different localities. Some parts of the city are built upon what is called "made land," resulting in many cases from the filling up of old creek bottoms and from other extensive levelling and grading. A more careful study of these peculiarities and their distribution may lead to the discovery of some relation between local differences in structure and the areas of greatest destruction.

Unquestionably much is to be attributed to the difference in the character of the buildings themselves, and to the relation of their lines of greatest or least strength to the direction of the wave front. As was to be expected, buildings constructed of wood suffered much less than those of brick. The interior of wooden buildings, however, would often exhibit a scene of total destruction, furniture, book-cases, &c., having evidently been moved with great violence. A very brief examination of injured buildings sufficed to establish, in a general way, the principal direction of the movement, which was probably in a north-west and south-east line.

The probability of the destruction of a building depends so largely on conditions other than the amplitude or direction of the vibration of the earth particle that the study of destroyed or damaged structures can yield little exact information concerning these elements. The displacement of bodies of simple form and structure, lying near to or upon the surface of the earth itself, is a vastly more reliable index of the direction and intensity of the disturbance. In the churchyards of Charleston many instances of displacement and overturned monuments, columns, urns, &c., were found. These were examined with some care, and a careful study of the results may bring out some information concerning the dynamics of the earthquake. A cemetery containing many pyramidal or cylindrical shafts resting upon flat stone bases is tolerably certain, when disturbed by an earthquake, to exhibit not only displacement but also instances of twisting about a vertical axis; cases of this kind were numerous at Charleston. Such rotations by no means imply a similar gyratory motion of the earth, as it is well known that they may result, and doubtless always do, from vibratory motions in a single plane. It was not at all uncommon to find two columns, very near to each other, twisted in opposite directions.

A table was given containing a *résumé* of information received at the office of the Chief Signal Officer from regular observers of the Service and from a number of voluntary observers. The place, time, supposed direction, duration, and estimated intensity were given. Much discrepancy is observable in the records of time. Confusion is especially great in a few portions of the country in which so-called "local time" is still adhered to. Whenever "standard time" is known to have been used reduction has been made to that of the 75th meridian. In a few cases, however, no reasonable supposition can explain the discrepancies. Such records must be erroneous.

A study of this column will show the great importance, in making such observations, of determining the error of the clock or watch at the earliest possible moment by comparison with the time of some known meridian. It must be said, however, that the extended use of standard time has rendered these results vastly more accurate than they otherwise would have been. Telegraphic time-signals are now within the reach of most people, and during the past two or three years a great improvement in the accuracy of time-keeping among the people has taken place.

The direction of the movement recorded against each station is that given by the observer. As it is based in many instances on the motions of swinging objects, or easily movable objects, it is of necessity often erroneous. In the absence of correct instrumental records, however, such observations are of value. The numbers expressing the intensity of the disturbance were applied at this office, from descriptions furnished by observers, according to a scale adopted by the Director of the Geological Survey.

This scale is as follows:—

No. 1. Very light. Noticed by a few persons; not generally felt.

No. 2. Light. Felt by the majority of persons; rattling windows and crockery.

No. 3. Moderate. Sufficient to set suspended objects, chandeliers, &c., swinging, or to overthrow light objects.

No. 4. Strong. Sufficient to crack the plaster in houses, or to throw down some bricks from chimneys.

No. 5. Severe. Overthrowing chimneys and injuring the walls of houses.

With these intensity numbers an attempt has been made to plot a chart of iso-seismal lines, or lines of equal intensity. The result is shown in the chart. Nothing short of the use of well-constructed seismographs can furnish satisfactory measures of the amplitude of vibrations of the earth particle or the maximum velocity of the same, but in the absence of records of such instruments, this chart, or a more perfect one constructed upon the same plan, will afford opportunity for study.

In conclusion, it ought to be stated that this brief review of the Charleston earthquake must be regarded only as an attempt to place some of the leading facts upon record, for the benefit of the readers of the *Monthly Weather Review*. It is in no way intended to anticipate the investigations now in progress by the United States Geological Survey, a full report from which, based upon all attainable information, will be looked for with great interest.

THE SIMILARITIES IN THE PHYSICAL GEOGRAPHY OF THE GREAT OCEANS[1]

At the outset Mr. Buchanan reminded the audience of the similarities observed in the eastern and western continents, especially in their southern extremities. Such similarities in corresponding localities had been called homologous geographical features, in imitation of the homologies of comparative anatomy, and they had received much attention from students of geography. A remarkable group of similarities of this kind is to be found in the arrangement of inclosed seas lying to the northward of the three southern continents. To the northward of South America there are the Gulf of Mexico and the different basins of the Caribbean Sea; to the northward of Africa there are the Mediterranean with its different basins, and on the north-east the Red Sea; and to the northward of Australia there are the well-known seas of the Eastern Archipelago. These seas are bounded on all sides by islands and insular groups, and they are in continuous connection with two oceans, the Pacific and the Indian. The African seas are bounded entirely by continental land and communicate directly with two oceans; but in the limited sense that one sea, the Red Sea, communicates with the Indian Ocean by a single channel, and the Mediterranean Sea with the Atlantic, likewise by a single channel. Finally, the American seas are all in continuous communication with only one ocean, the Atlantic, the continental barrier towards the Pacific being continuous.

It is not unworthy of remark that the great depths (over 4000 fathoms) of the Atlantic and the Pacific Oceans occur immediately to the northward of these groups of seas, and in the western sinus of the northern portions of both oceans; while the greatest depression of the continental land, the region of the Dead Sea, is found similarly situated with regard to Africa. The analogy here, however, does not hold good all through, because it is a mere accident of climate that this area does not form a large and not excessively deep fresh-water lake.

The seas of the Malay Archipelago and those of the West Indies have important functions in the physical geography of the oceans, as they receive the warm dense water of the westerly-running equatorial currents of the Pacific and the Atlantic Oceans. The Pacific current finds no obstacle in the chains of islands which bound the Malayan seas, and is able to pass freely through into the Indian Ocean; while the Atlantic current is stopped by the continuous continental barrier of South America, and the head of water thus produced is relieved by the overflow of the Gulf Stream all the year round. Although there is no static barrier, in the shape of continuous land, to the westerly Pacific current, there is, during one season of the year, a kinetic one, furnished by the prevalence of the south-west winds during the monsoon season. These furnish the intermittent *kuro siwo*. The main cause of the westerly equatorial current is the propulsive action of the trade winds.

These winds have also great evaporating power; and, by making the surface water salter, they furnish the mechanical means of propagating the surface heat into the deeper layers of the ocean. Hence the leading characteristic of the westward or leeward regions of the intertropical oceans is water of considerable density and of high average temperature in the sub-surface layers. This characteristic is seen most clearly in the Atlantic, where there is no communication with another ocean. In the Pacific the non-continuous boundary neutralises to some extent this effect, and gives to the eastern parts of the Indian Ocean a borrowed leeward character, independent of its own climate. A secondary consequence of a leeward position in the ocean, and due to the above-mentioned characteristics of the temperature and density of such water, is the prevalence of coral formations in the western regions of the Atlantic and Pacific, and, owing to the mixture of conditions, in both eastern and western regions of the Indian Ocean.

Continental homologies, or similar features in corresponding localities, are found on the western as well as on the eastern sides of the continents. One of the most striking is the resemblance of the Gulf of Guinea on the African coast with the great Central American bight stretching from Cape St. Lucas at the extremity of the Californian Peninsula, by Panama, to the mouth of the Guayaquil River, and with the unnamed bight in the Indian ocean bounded continentally by the north-west coast of Australia and insularly by the chain of islands stretching from the Peninsula of Malacca to Australia. Oceanically these bights are homologous. It is in them that the beginnings of the westerly-running equatorial currents are to be found, and perhaps more important still, it is in them that the easterly-running counter equatorial currents end. They are to be found in each of the three oceans, and generally on the northward side of the axis of the westerly-running current. In the Atlantic it is best known by its eastern portion, the Guinea current.

The observations here recorded of the Guinea current, a hitherto unexplored region of the ocean, were made on board the steamship *Buccaneer*, at the invitation of the owners, the India-rubber, Gutta-percha, and Telegraph Works Company, of Silvertown, and were carried out during a survey for a telegraph cable from Sierra Leone to St. Paul de Loanda. From a diagram showing the variation of salinity of the surface water of the Guinea current, with distance from the coast, it appeared that for a considerable distance along the Guinea coast the salinity of the surface water was an almost accurate test of the proximity of the land. The Guinea current starts in mid-ocean, but it is most constant near the African coast. The density of the water is low, its temperature high, and its velocity, especially in-shore, is sometimes as great as three miles an hour. It varies somewhat with the season.

Bottle experiments showed an average rate of fifteen miles per day in the months of January and February, for a thousand miles along the coast. In March, the *Buccaneer* experienced no easterly current, and in connection with this absence of easterly currents off the coast may be taken the very remarkable under-current which is found setting in a south-easterly direction with a velocity of over a mile per hour at three stations almost on the equator, and to the northward of the Island of Ascension. For the double purpose of examining the currents and of obtaining a large specimen of the bottom, the *Buccaneer* was anchored in 1800 fathoms of water by means of an ordinary light anchor fitted with a canvas bag to receive the mud which would otherwise fall off the flukes on its being weighed. While the ship was lying thus at anchor, the surface water was found to have a very slight westerly set. At a depth of 15 fathoms there was a difference, and at 30 fathoms the water was running so strongly to the south-east, that it was impossible to make observations of temperature, as the lines, heavily loaded, drifted straight out, and could not be sunk by any weight the strain of which they could bear. In the Pacific the counter equatorial current in the open ocean was well observed by the

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[1] Abstract, by the Author, of a Paper read at the meeting of the Royal Geographical Society on Monday, November 8, by Mr. J. Y. Buchanan.

Challenger on her voyage from Hawaii to Tahiti. Her observations were illustrated by two diagrams, one showing the direction of the current, and the other the distribution of temperature and density in the upper layers of the water traversed. The easterly current was found between the parallels of 5° N. and 10° N., there being two streaks of maximum velocity, one between 7° and 8° N., and the other between 9° and 10°. In the former the mean daily set was 54 miles; in the latter it was probably quite as high, but it could not be accurately determined, as the ship passed from westerly to easterly current in the course of the 24 hours, and the observed current of 20 miles represented the difference of the two. The streaks or axes of strong easterly current are sharply defined by areas of abnormally low surface density. The whole of the area of easterly-running water has a comparatively low density, but where there is a sudden acceleration of its velocity, there is a correspondingly sudden drop in its density, so that the existence of a strong easterly current in equatorial regions may be guessed with great probability by the use of the hydrometer. The diagram showed also in a very marked way the protective action of the fresh surface water in preventing the penetration of heat into the lower layers of the water. A temperature of 60° Fahrenheit is found here at a depth of 50 fathoms from the surface, while in the westerly-running current, a little further south, the same temperature occurs at a depth of over 100 fathoms. In this region there are great inequalities in the density of layers of water at the same depth and within a short distance of each other. Thus, if the column of water between 20 fathoms and 70 fathoms from the surface be considered, its weight at the station where the westerly-running equatorial current prevails is only 88 per cent. of its weight under the counter equatorial current, the distance between them being not more than 200 miles. This disturbance of statical equilibrium must be balanced by circulation of water between the localities, and hence the violent and conflicting currents observed in these regions. The study of the currents of equatorial regions would well repay the trouble of the investigation. The counter equatorial current is particularly interesting, and its dynamics obscure. Its range is very superficial, and its physical conditions can be studied without the elaborate and costly equipment required for the research of oceanic depths.

To the north and to the south of the equatorial bights of the western shores of Africa and America we have a remarkable similarity in the distribution of temperature in the coast waters. The transition from equatorial heat to extratropical cold is very marked: on the North American shore, at Cape St. Lucas, the southern extremity of the Californian peninsula; on the North African, at Cape Verd; on the South American shore, at Cape Blanco; and on the South African, at Cape Frio. In rounding Cape St. Lucas the temperature was observed to fall from 75° to 65° F. in less than an hour; and a similar difference of temperature was found in rounding Cape Blanco between Payta and the Guayaquil river. On the Morocco coast the water is found to have a temperature quite 10° lower than is found twenty miles to sea. These sharp transitions are found only close inshore, and they have usually been attributed to surface currents from higher latitudes. This explanation is at variance with the observations of navigators on the coasts, who do not notice any currents which would be strong enough to bring water many hundreds of miles under a burning sun without sensible rise in temperature. The occurrence of these coast areas of abnormally cold water is explained when we recognise that they are the windward shores of the oceans. The trade winds blow from them towards the equator, and in doing so mechanically remove water, which has to be supplied from the readiest source. This source is the deep water lying off the continental coasts, which is supplied by a gradual drift of cold water from high latitudes. Hence, though the low temperature of the coast waters referred to is due to the cold of high latitudes, it is not supplied by a long coast Polar current, but by a short vertical one. This view was very strongly supported not only by the temperature of the water, but by its other characteristics, especially colour. The outside ocean water is of an intense ultramarine blue; the coast water off Mogador had the clear olive-green colour met with constantly in Antarctic seas. The same is observed on the west coast of North and South America, and it would be of the highest interest to have these waters investigated from a biological point of view. No waters in the ocean so teem with life as those on the west coast of South America. A bucket of water collected over the side is turbid with living organisms, the food of countless shoals of fish, who, in their turn, afford prey for innumerable schools of porpoises. One remarkable school which accompanied the ship for some time consisted entirely of females, each accompanied by a calf following in her wake and mimicking her every movement. Along with abundance of life this coast unites facilities for investigating it. At every port there are plenty of shore boats anxious for a fare, and with a tow-net and a few bottles a naturalist might make a rich collection of the shore-water fauna of the coast in one trip from Valparaiso to Panama.

The most remarkable confirmation of the view that the cold water on the windward shores is due to a submarine source has been quite recently supplied by the observations of Capt. Hoffmann, of the German man-of-war *Möwe*, on a voyage from Zanzibar to Aden. He kept close to the coast as far as possible, and observed a very uniform surface temperature of 78° to 80° F. from Zanzibar to Cape Warschek, when it began to fall, and remained at a temperature of from 60° to 65° F., until Cape Guardafui was reached, when the temperature went up rapidly to 86°. The minimum temperature observed was 59° F., and Capt. Hoffmann calls particular attention to the dark-green colour of the water, and in speaking of its low temperature he recognises that its source can only be the deep water in the neighbourhood, as the surface water on both sides has a temperature bordering on 80° F. The *Möwe* passed through these seas in the month of July, when the south-west monsoon is blowing most strongly, and at this season the Somali coast is a pronounced windward shore, and exhibits the same characteristics as the windward shores of Morocco or South America. The coral growths, too, which are so abundant north and south of it are here quite absent, thus accentuating the eastern or windward character of the shore.

NOTES

The following is the list of selected names to be submitted to the Fellows of the Royal Society at the forthcoming anniversary meeting (November 30) for election into the Council for the ensuing session:—President: Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.; Treasurer: John Evans, D.C.L., LL.D.; Secretaries: Prof. Michael Foster, M.A., M.D., Lord Rayleigh, M.A., D.C.L.; Foreign Secretary: Prof. Alexander William Williamson, LL.D.; other Members of the Council: Prof. Robert B. Clifton, M.A., Prof. George Howard Darwin, M.A., LL.D., W. T. Thiselton Dyer, M.A., Prof. David Ferrier, M.A., Edward Frankland, D.C.L., Arthur Gamgee, M.D., Archibald Geikie, LL.D., Prof. Joseph Henry Gilbert, M.A., John Hopkinson, M.A., D.Sc., J. Norman Lockyer, F.R.A.S., Sir Lyon Playfair, K.C.B., LL.D., Prof. Bartholomew Price, M.A., Prof. Pritchard, M.A., Admiral Sir George Henry Richards, K.C.B., Prof. Arthur Schuster, Ph.D., Philip Lutley Sclater, M.A., Ph.D.

In the third volume of Ray's "Historia Plantarum" there is a list of plants collected in the Island of Luzon by George Joseph

Camelli. This botanist was a member of the Society of Jesus, and was born at Brunn, in Moravia, April 21, 1661; after a life spent for the most part in the Philippines, he died at Manila, May 2, 1706. Linnæus commemorated him in the genus Camellia, and the introduction of this well-known plant into Europe is generally attributed to him. The manuscript transmitted by Camelli to Ray was accompanied by a large number of drawings, part only of which Ray seems to have been able to afford the expense of publishing. We learn from the *Comptes rendus* of the Société Royale de Botanique de Belgique for October 9, 1886, that the whole of the drawings still exist in a folio volume in good preservation in the library of the Jesuits' College at Louvain. It contains 257 autograph plates, with 556 figures of plants, and three plates, with nine figures relating to zoology. It was purchased at the sale of the library of Antoine Laurent de Jussieu (February 6, 1858), in whose handwriting it is carefully annotated, and was presented to the Jesuit College by Count Alfred de Limminghe.

DR. WALTER L. BULLER, C.M.G., F.R.S., the well-known New Zealand ornithologist, has been promoted to the Knighthood of the Order of St. Michael and St. George.

THE honorary degree of D.Sc. has been conferred by the Senate of the Royal University of Ireland upon the Rev. S. J. Perry, F.R.S., and Prof. John Perry, F.R.S.

THE Committee appointed by the Prince of Wales to assist in framing a scheme for the proposed Imperial Institute is ludicrously inadequate and unrepresentative. The President of the Royal Academy appears, but why is the President of the Royal Society omitted? Surely science will have far more to do with such an institute than art. The only representative of science is Sir Lyon Playfair, and he has been appointed probably more on account of his connection with the 1851 Exhibition than with science. If the Committee is to gain the confidence of the public it must be of a very different character.

IN view of the progress achieved of late in the domain of celestial photography, the French Academy of Sciences has decided to propose that an International Conference be held in Paris next spring to make arrangements for the elaboration of a photographic map of the heavens to be simultaneously executed by ten or twelve observatories scattered over the whole surface of the globe.

ACCORDING to the Report of the Director of the Leander McCormick Observatory of the University of Virginia for the year ending June 1, 1886, the buildings and instruments are in excellent repair; the great 45-feet dome revolving fully as easily as when first erected. The Parkinson and Frodsham clock, formerly belonging to the Physical Laboratory, has become the property of the Observatory. It is now in Washington, in the hands of a jeweller, to be cleaned and recased. The great equatorial has been chiefly employed in the examination and sketching of southern nebulæ. The nebula in Orion and the Trifid and Omega nebulæ have received special attention. 351 observations of miscellaneous nebulæ have been made, resulting in 226 drawings, and the discovery of 233 nebulæ which are supposed not to have been hitherto detected. The features seen indicate that the performance of the instrument employed surpasses that of any of the great reflectors which have been used in the examination of nebulæ, the examination of complicated structures seldom failing to show features not noticed elsewhere. Only a few nights have been suited to the micrometrical measurement of double stars. Seventy-six observations have, however, been made of stellar pairs, nearly all of which are close and difficult. According to the Director, Mr. Ormond Stone, the past year has been, without exception, the poorest for astronomical observations which he has ever known. Not only have there been an unusual number of cloudy nights, but even on clear nights the definition has been almost always extremely poor. The Observatory is open to the general public every day, except holidays and Sundays, between 2 and 5 p.m. It is also open to a limited number of visitors once each month at 8 p.m.

BY the kindness of the under-mentioned gentlemen, lectures will be delivered as follows before Christmas at the Royal Victoria Hall and Coffee Tavern:—November 16, Mr. A. T. Arundel (Madras Civil Service), "Glimpses of India and its People"; November 23, Mr. Arthur Brown, "The Yellowstone Region"; November 30, Prof. A. W. Rücker, "Early History of the Earth and Moon"; December 7, Rev. W. H. Dallinger, "Plants that Prey on Animals and Animals that Fertilise Plants"; December 14, Prof. Boyd Dawkins, "Introduction of the Arts into Britain." With regard to the classes now held in the building, about eighty students have joined, many of whom are attending more than one class, and it is expected that fresh classes will shortly be started. A very satisfactory feature of the matter is that the students are genuine artisans, who would not otherwise have good teaching within their reach.

IN the form of a leaflet reprinted from *Humboldt* (Band v. Heft 10), M. Habenicht, of Gotha, sends us a "Contribution to the Morphology of the Kosmos." Although his emendation of the nebular hypothesis can scarcely be called an improvement upon it, it is one among many symptoms of the breaking up of ideas on the subject, and their tendency to flow into new channels. M. Habenicht remarks that, in the primitive nebula, "the laws of Nature slumber." For the convenience of the majority of speculators on origins, their awakening should be indefinitely postponed. His theory of planetary formation depends upon disparity of temperature, the inner side of the originating ring being warmed by the central body, while the outer side radiates freely into space. The result is unequal contraction occasioning rupture at the weakest place, whereupon a remarkable process ensues. Through the *tightening* of its outer surface, the ring coils up from the outside into two spirals containing very different quantities of matter, which eventually rush together from opposite directions, and coalesce into a planet. This dual origin is visible in the dissimilarity of the terrestrial hemispheres, as well as in certain aspects of Mars, and in some rare glimpses by Dawes of the disposition of light and shade on Jupiter's third satellite. The analogy is even carried out, we are told, in the organic world, from the tiny seed-leaves of the embryo-plant to the symmetrical yet not strictly balanced arrangement of limbs in the highest order of beings. But the planet-producing rings, to behave as M. Habenicht supposes them to have behaved, should have possessed rather the qualities of caoutchouc than those of any known or imaginable "nebulous" stuff.

UNDER the title of "Sea-Level and Ocean-Currents," Prof. J. S. Newberry sends the following letter to *Science*:—"Put-in Bay Island, October 16, 1886.—At 11 o'clock Thursday evening, the 14th inst., I witnessed here a remarkable fact, the effect of the late tremendous wind-storm. This commenced about 7 a.m., and began to let up at 11 o'clock in the evening, or a little later. I then went down to the shore in front of my house, and found the lake lower than the average by fully 6 feet! This is the greatest depression from such cause I have noticed during a residence here of nearly twenty-four years. We have not, within this period, had such a high wind steadily continued for so long a time. The captain of the steamer *Chief Justice Waite*, running between Toledo and the islands, reports the fall of water-level at Toledo as about 8 feet." In discussing the general question with reference to previous correspondence, Prof. Newberry says:—"The question is, not whether the wind has the power of raising the water-level on a coast, but whether wind-friction can, in the great equatorial belt and in the track of the Gulf Stream, produce the flow of water which is there observed. The striking cases of the power of wind to heap water on coasts, and to move bodily great masses of it in lakes, are only interesting and relevant as demonstrating the sufficiency of wind-friction to produce broad and rapid surface-currents. This conceded, and the case is won, because, in the lakes and open ocean, like causes produce like effects. Wind of given velocity raises in both places waves of equal height in equal times: against these waves the wind presses in the direction of its flow, with no opposing force. As a consequence, the roughened water-surface, from greatly increased friction, is moved bodily forward just as though impelled by the paddles of a revolving-wheel. This surface-flow is in time communicated to underlying strata, and, if the wind continue to blow in the same direction, ultimately a large body of water will be set in motion; in other words, an ocean-current will be produced. There is no escape from this conclusion. The great truth remains that wind-friction can produce ocean-currents."

A SHOCK of earthquake of a more or less severe nature was felt at noon on November 5 at Washington, Richmond, Wilmington, Raleigh, Augusta, Charleston, Savannah, Macon, and other places in North and South Carolina. At some points the seismic disturbance was the severest since August 31. A shock of earthquake was also felt at Greenville, Alabama, on Friday last. The captain of a vessel which has since arrived at Charleston reports having experienced a seismic disturbance on that day while at sea.

PROF. JOHN MILNE, of Tokio, Japan, writes with reference to Prof. Ewing's article on seismographs in NATURE, vol. xxxiv. p. 343, that the instruments therein described represent the state of general knowledge of the Seismological Society of Japan with regard to seismometry at the time of Prof. Ewing's departure from that country. With the exception of one or two which have been modified, a set of instruments like those recommended by Prof. Ewing are, so far as Japan is concerned, quite obsolete. A very much better form of instrument is Prof. Milne states, now in use in the Government observatories and throughout the country.

IN a paper by the Hon. Ralph Abercromby, reprinted from the *Quarterly Journal* of the Meteorological Society, on the origin and course of the squall which capsized H.M.S. *Eurydice* on March 24, 1878, the author concludes as follows:—"The squall which capsized H.M.S. *Eurydice* was one belonging to the class which is associated with the trough of V-shaped depressions. The line of this trough was curved like a scimitar, the convexity facing the front. The whole revolved round a point near the Scaw, in Denmark, like the spoke of a wheel. For this reason the portion of the squall over the east of England moved only at the rate of 13 miles an hour, while the western portion travelled nearly 50 miles in an hour. The portion which struck the *Eurydice* was advancing at the rate of 38 miles an hour. The length of the squall over England was more than 400 miles, but only 1 to 3 miles in breadth. Hence we have the picture of a scimitar-shaped line of squalls, 400 miles long and about 2 miles broad, sweeping across Great Britain at a rate varying from 13 to 50 miles an hour. The V-depression was one of an uncommon class, in which the rain occurs after the passage of the trough, and not in front of it, as is usually the case. The weather generally for the day in question was unusually complex, and of exceptional intensity, and for this reason some of the details of the changes cannot be explained."

AT a recent meeting of the Niederrheinische Gesellschaft für Natur- und Heilkunde at Bonn, Dr. Gurlt described a fossil meteorite found in a block of Tertiary coal, and now in the Salzburg Museum. He said it belonged to the group of meteoric irons, and was taken from a block of coal about to be used in a manufactory in Lower Austria. It was examined by various specialists, who assigned different origins to it. Some believed it to be a meteorite; others, an artificial production; others, again, thought it was a meteorite modified by the hand of man. Dr. Gurlt, however, came to the conclusion, after a careful examination, that there is no ground for believing in the intervention of any human agency. In form, the mass is almost a cube, two opposite faces being rounded, and the four others being made smaller by these roundings. A deep incision runs all round the cube. The faces and the incision bear such characteristic traces of meteoric iron as to exclude the notion of the mass being the work of man. The iron is covered with a thin layer of oxide; it is 67 mm. high, 67 mm. broad, and 47 mm. at the thickest part. It weighs 785 grammes, and its specific gravity is 7·75; it is as hard as steel, and it contains, as is generally the case, besides carbon, a small quantity of nickel. A quantitative analysis has not yet been made. This meteorite resembles the celebrated meteoric masses of Saint Catherine in Brazil and Braunau in Bohemia, discovered in 1847, but it is much older, and belongs to the Tertiary epoch.

DR. DOBERCK, the Government Astronomer in Hong Kong, has published a pamphlet entitled "The Law of Storms in the Eastern Seas," containing the practical results of investigations of about forty typhoons, continued during three years. He divides typhoons into four classes, according to the paths which they usually follow:—(1) Those which cross the China Sea and travel either in a west-north-westerly direction from the neighbourhood of Luzon towards Tonquin, passing south of, or crossing the Island of Hainan; or, if pressure is high over Annam, they travel first westward and then south-westward. These, which occur at the beginning and end of the typhoon season, can generally be followed for five or six days. (2) The second class are most frequently encountered, and their paths can be traced farthest. They generally travel north-westward while in the neighbourhood of Luzon, and either strike the coast of China south of the Formosa Channel, in which case they abruptly lose the character of a tropical hurricane, re-curve in the interior of China, and re-enter the sea to the north of Shanghai, pass across or near Corea, and are finally lost to the east-north-east. Typhoons of this class may pass up the Formosan channel, and re-curve towards the coasts of Japan, or they may strike the coast of China north of Formosa. A third of the typhoons belong to this class; they can be followed between five and twelve days, and are most common in August and September. (3) This class is probably the most numerous of all, although not so frequently encountered. Their path is along the east of Formosa, travelling northwards and passing near Japan. (4) Typhoons of this class pass south of Luzon, travelling westward. Their dimensions are very limited, and hitherto they have not been followed for more than a day or two. When a few hundred typhoons have been investigated, no doubt complete lists of the sub-classes of these four main classes will be obtained, and exceptional cases will be better understood. The pamphlet, which is largely written for the guidance of ship-masters and others, concludes with the remark that typhoons are of simpler construction, and their paths are more regular, than the storms of Europe. Typhoons are so violent near their centre that the whole disturbance is evidently ruled thereby; whereas storms in the North Atlantic and in Europe appear to be made up of a number of local eddies, some of which are by degrees detached from the chief disturbance and form subsidiary depressions. Dr. Doberck has not been able to ascertain the existence of a subsidiary depression in the China Seas during the last three years, and it is, therefore, doubtful whether they ever occur.

The additions to the Zoological Society's Gardens during the past week include two Rhesus Monkeys (*Macacus rhesus* ♀ ♀) from India, presented respectively by Col. J. M. McNeile and Mrs. E. White; a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, presented by Miss Townshend Wilson; twelve Barbary Turtle Doves (*Turtur risorius*) from Africa, presented by Mr. E. L. Armbrecht, F.Z.S.; four Copper-head Snakes (*Cenchris contortrix*), two Rattlesnakes (*Crotalus durissus*), a Hog-nosed Snake (*Heterodon platyrhinos*) from North America, presented by Mr. W. A. Conklin, C.M.Z.S.; a Long-nosed Snake (*Heterodon nasicus*) from Indiana, U.S.A., presented by Miss Catherine Hopley; a Fire-bellied Toad (*Bombinator igneus*) from Germany, presented by Mr. G. A. Boulenger, F.Z.S.; a Bactrian Camel (*Camelus bactrinus* ♂), bred in England, two Eleonora Falcons (*Falco eleonoræ*) from North Africa, a Macaque Monkey (*Macacus cynomolgus* ♂) from India, deposited; two Mantchurian Crossoptilons (*Crossoptilon mantchuricum* ♂ ♀), two Bar-tailed Pheasants *Phasianus reevesi* ♂ ♀) from Northern China, purchased; ten Barbary Turtle Doves (*Turtur risorius*), four Ring Doves (*Turtur communis*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

Stellar Photography at Harvard College.—Prof. Pickering has recently presented to the American Academy of Arts and Sciences an important memoir on the work in stellar photography which has been carried on at Harvard College, mainly by aid of an appropriation from the Bache Fund. The memoir commences with a brief sketch of the history of stellar photography, from its origination in 1850, when Mr. J. A. Whipple succeeded in obtaining a satisfactory daguerreotype of Vega with the Harvard 15-inch equatorial, the first stellar photograph ever secured. In 1857, the collodion process having then been introduced, Prof. G. P. Bond resumed the investigation, and showed that photography was capable of doing real work in the observation of double stars. In 1882 some preliminary experiments with a lens of 2½ inches aperture were made, and with such satisfactory results that in 1885 the work was resumed with a Voigtländer lens of 8 inches aperture, and about 45 inches focal length, that focal length having been selected that the photographs might correspond in scale to the maps of the "Durchmusterung." Of the three departments into which stellar photography may be divided, viz. star-charting, photographing star-trails, and spectrum photography, Prof. Pickering has chiefly interested himself in the two latter. Star-trails—the images, that is, produced on a plate when the telescope is stationary, or is not following the star with precision—are made exceedingly useful. It furnishes the best method of determining the magnitudes of stars photographically, and the average deviation of the measures of the brightness of circumpolar stars on different plates proved to be less than a tenth of a magnitude, a greater accordance than is given by any photometric method. It is Prof. Pickering's intention to obtain determinations of the brightness of all stars north of 30° S. decl. by this method, and the work is now nearly completed. One of the plates taken on November 9, 1885, incidentally affords conclusive evidence that Mr. Gore's Nova Orionis was then much less bright than it was on the night of its discovery, some five weeks later. By photographing on the same plate circumpolar stars near their upper and lower culminations, the means for determining the atmospheric absorption on the nights of observation have been secured. Prof. Pickering has also made some experiments on the applicability of photography to the transit instrument, and concludes that the position of a star may be determined from its trail with an average deviation of only 0·03s. Prof. Pickering also shows how star-trails may be made useful in determining the errors of mounting of the photographic instrument. Photographs of stellar spectra have been obtained by simply placing a large prism in front of the object-glass. The spectra of all the stars over an extended area are thus obtained at a single exposure; an exposure of five minutes giving the spectra of all stars down to the sixth magnitude in a region 10° square. The entire sky north of 23° S. decl. is to be examined in this way, and the work is now far on the way to completion. An exposure of an hour shows the spectra of stars down to the ninth magnitude. A photograph of the Pleiades in this manner brings out the interesting fact that, with very few exceptions, all have spectra of the same class—a circumstance which seems strongly to confirm the idea of a community of origin. The exceptions may not improbably lie at a considerable distance on this side or the other of the group, and should, as Prof. Pickering suggests, receive attention in any study of the parallax of the Pleiades. Prof. Pickering also here discusses several theoretical points of interest, one being the relation between the dimensions of the lens employed and the light of the faintest star that can be photographed with it. He concludes, on the whole, that, where the telescope follows the star with exactness, the limiting amount of light may be assumed as proportional to the aperture divided by the square root of the focal length. Three photographic plates accompany the memoir: the first showing the photographic instrument, the second the trails of a number of close circumpolar stars, and the third several specimens of photographs of stellar spectra, those of Vega, Altair, and of the Pleiades being amongst the number.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 NOVEMBER 14-20

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 14

Sun rises, 7h. 18m.; souths, 11h. 44m. 35·5s.; sets, 16h. 11m.; decl. on meridian, 18° 18′ S.: Sidereal Time at Sunset, 19h. 46m.

Moon (at Last Quarter November 18) rises, 17h. 54m.*; souths, 1h. 43m.; sets, 9h. 37m.; decl. on meridian, 18° 13′ N.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	9 37	13 17	16 57	25 11 S.
Venus	6 49	11 27	16 5	16 16 S.
Mars	10 39	14 23	18 7	24 36 S.
Jupiter	4 35	9 57	15 19	8 15 S.
Saturn	20 2*	4 4	12 6	21 19 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Star by the Moon (visible at Greenwich)

Nov.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
14	115 Tauri	6	4 2	5 11	139 295

Nov. h.
16 ... 13 ... Saturn in conjunction with and 3° 3′ north of the Moon.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Nov. 18,	3 8	*m*
R Arietis	2 9·6	24 31 N.	,, 18,		*m*
Algol	3 0·8	40 31 N.	,, 14,	0 45	*m*
			,, 16,	21 34	*m*
ζ Geminorum	6 57·4	20 44 N.	,, 18,	21 30	*m*
U Canis Minoris	7 35·2	8 39 N.	,, 18,		*M*
R Virginis	12 32·7	7 37 N.	,, 18,		*m*
S Ursæ Majoris	12 39·0	61 43 N.	,, 14,		*m*
U Virginis	12 45·3	6 10 N.	,, 18,		*M*
R Scuti	18 41·4	5 50 N.	,, 17,		*m*
β Lyræ	18 45·9	33 14 N.	,, 16,	0 0	*M*
η Aquilæ	19 46·7	0 43 N.	,, 16,	19 0	*M*
δ Cephei	22 24·9	57 50 N.	,, 14,	0 0	*m*

M signifies maximum; *m* minimum.

Meteor Showers

November 14 is the date of the *Leonid* shower, R.A. 149°, Decl. 22° N.

THE EROSION OF THE ENGLISH COASTS

The opening meeting of the present session of the Geologists' Association took place last Friday evening at University College, when an address was delivered by Mr. W. Topley, President of the Association and Secretary of the British Association Committee on Coast Erosion. The subject of the address was "The Erosion of the Coasts of England and Wales."

Mr. Topley, in his address, referred to the great service rendered to the country by Mr. J. B. Redman, who had given much attention to the question of coast erosion, and to whom the British Association Committee was greatly indebted. The speaker then proceeded, by the aid of diagrams and drawings on the blackboard, to describe the mode in which the sea acts on coasts of various kinds, and stated the rate at which erosion is taking place in different parts of the country. It was greatest along the coast of Holderness and Norfolk, where the sea gained on the land at the average rate of from 2 to 3 yards per year. But locally and during exceptional gales the rate was much higher. On January 30, 1877, parts of Norfolk lost an average of 3 yards for several miles, and near Bacton the loss was 15 yards. Typical instances of erosion were cited, among the places mentioned being Folkestone, Brighton, Worthing, Bournemouth, Westward Ho! and Pembrokeshire. The speaker then went on to describe the shingle beaches and their changes, and to discuss the effects of natural and artificial groynes. On the south coast of England the shingle travelled from west to east, and if left to itself it would form a natural protection along the greater part of the coast, and the average amount of erosion would be small. But in certain places land-owners, town-councils, and other corporations desired that there should be no loss of land, and they erected groynes to collect the shingle, and so robbed the coast to the east of its natural protection. Worthing was heavily groyned and the shingle largely collected, but just east of the town the coast was rapidly receding. Folkestone pier was a large groyne which had collected an extensive area of shingle on its west side; Copt Point and Eastwear Bay, once protected by a continuous band of shingle, were now nearly bare, and the coast was rapidly going. At Copt Point land was laid out for building, and roads were made; but the notice-board advertising "this desirable freehold building land," was seen half-way down the cliff. Natural groynes were sometimes recklessly destroyed, and this was the case at Hengistbury Head, where ironstone was quarried from the cliff and foreshore; the reef had held back sufficient shingle to protect the land to the west, but when the reef was removed, the shingle travelled on, and the land rapidly receded. Great damage was done by taking shingle for road metal, ballast, or other purposes. The amount so taken appeared small and unimportant because a single storm might throw up as much as might be taken in many months, but the aggregate amount so removed was enormous, and must tell in time. It had been estimated that the shingle removed near Kilnsea in twenty years represented a bank 3 miles long, 31 yards wide, and 6 feet deep. It was interesting to note that the erosion of that part of the coast averaged only from three-quarters of a yard to a yard and a half per year for some time before the shingle trade was so largely developed; but later on, owing to the loss of the shingle, the rate of erosion rose from 3 to 6 yards per year. The change might not be entirely due to the cause mentioned, but it clearly was so to a large extent. Although the Board of Trade had now stopped the practice at that part of the coast, it was still in full action in a large number of places. The speaker then passed to the consideration of the land gained from the sea. A great part of the material worn from the coasts of Holderness and Norfolk was carried into the estuaries of the Humber and the Wash, and there formed banks of sand and silt of great hindrance to navigation, but when reclaimed of great agricultural value. Recent estimates showed that the area of land thus made in the Humber and Wash was far in excess of that lost. Taking the whole coast-line of England, it was probable that the total area of land was as great now as it was 500 years ago. Although the general result of a survey of this question was less serious than was generally supposed, it was evident that greater control was requisite over the action of land-owners and public bodies along the coast. The powers now vested in the Board of Trade might be more rigorously and systematically applied, or fresh powers obtained. This was especially desirable along the south coasts, as there the damage done by reckless groyning was enormous, but the area of land now gained was small.

OBSERVATIONS ON HEREDITY IN CATS WITH AN ABNORMAL NUMBER OF TOES

IN 1883 I contributed an article to NATURE (vol. xxix. p. 20) upon this subject, giving an account of my observations from 1879 up to the date at which the paper was written. The last observation was concerned with a family of four male tabby kittens, all of which possessed the abnormality to a very marked extent. This was the first family produced by a female tabby (and slight tortoiseshell) cat which, when born, was the most abnormal form which had come under my notice, possessing two extra toes on all the paws, *i.e.* seven on each fore-paw and six on each hind-paw. The right paws of this cat were figured in the paper referred to, together with the corresponding paws of a normal cat, for comparison. These figures are now reproduced in order to illustrate the present paper. I quote the description of the figures from the previous paper. "It is seen that the extra toes (in the fore-feet) are those labelled A and B (in Figs. 1 and 2), and they confer the extraordinary breadth upon the foot. The most recently added is B, which is still

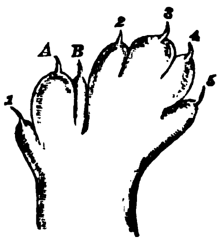

FIG. 1.—Right fore-paw from above, with extra toes.

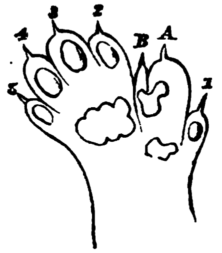

FIG. 2.—Right fore-paw from below, with extra toes.

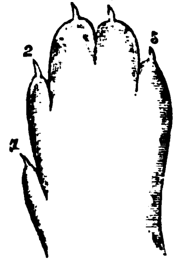

FIG. 3.—Right fore-paw from above, normal.

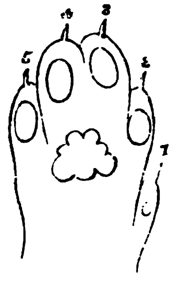

FIG. 4.—Right fore-paw from below, normal.

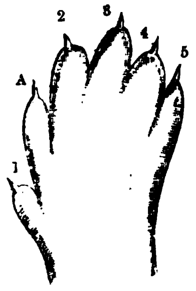

FIG. 5.—Right hind-paw from above, with extra toes.

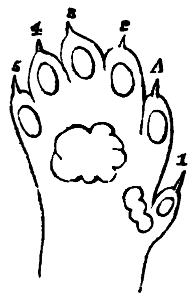

FIG. 6.—Right hind-paw from below, with extra toes.

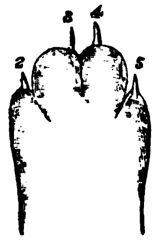

FIG. 7.—Right hind-paw from above, normal.

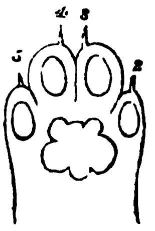

FIG. 8.—Right hind-paw from below, normal.

partially coalesced with A, and has but one pad in common with it (Fig. 2). . . . There is seen to be an extra pad behind the additional toes, of which there is no trace in the normal foot." In some families to be described, and also in two previously noted, the large extra toe, A, is present, while the insignificant pollex (Fig. 1, 1) is absent, and thus the paw appears extremely broad, although with only the normal number of toes. In the hind-paws (Figs. 5 and 6) "there is little doubt that the innermost toe 1 is the hallux lost in the normal foot. . . . The second extra toe is that labelled A. . . . On the under side (Fig. 6) all the toes have separate pads, and there is an additional pad behind the extra toes," which is sometimes fused with that behind the normal toes.

This cat produced her first family, described in the previous paper, on July 10, 1883. Continuing the observations from that date, the next family (of four tabby kittens) was born in June 1884. (1) and (2) were normal—a male and a female. (3)—a female—possessed six toes on the fore-paws, each toe with a separate front pad, and a bifid hind pad (distinct from that for the other toes) to the two inner toes (1 and A in Figs. 1 and 2); the toe shown in the figure and absent in this kitten is of course that marked B—the last to be added in all cases. The hind-paws possessed six toes each, as in the mother, and with the same arrangement of pads as in her left hind-paw, *i.e.* with separate front pads to each toe (as in Fig. 6), but with the hind pads for the extra toes 1 and A continuous with those for the four normal toes (unlike Fig. 6 in this respect). (4), a female, possessed seven toes on the right fore-paw; the front pads separate except in the case of those for the toes A and B, Fig. 2, which were slightly fused. The hind pad for the three innermost toes was quite separate from that for the others. This paw, in fact, almost exactly resembled that of the mother-cat on the same side, shown in Figs. 1 and 2. The left fore-paw possessed six toes, the small one marked B in Figs. 1 and 2 being absent. The pads were in other respects similar to those of the right paw. Thus the relative amounts of abnormality on the two sides are as with the mother, the preponderance being on the right side in both cases. But the difference is here greater in both directions, the right paw having rather more abnormality than in the mother, because of the less complete fusion between the front pads of the toes A and B, while on the left side the abnormality is much less than in the mother, in the complete suppression of the toe B. The hind-paws were as in the last kitten, and similar to the left hind-paw of the mother.

The next family (of three) was born September 22, 1884. (1), a female tabby kitten, was normal. (2), a female tabby kitten, possessed seven toes on the right fore-paw, with separate front pads to each toe and the hind pad as in Fig. 2. The innermost claw was double, the two divisions being arranged vertically one above the other, the lower being small and incomplete. In this respect, and in the separate front pad to the toe B, this paw is far beyond the mother's paw of the same side in abnormality. The left fore-paw possessed six toes, that marked B being absent. Otherwise the arrangement of pads was similar to that shown in Fig. 2. Hence this paw is more normal than that of the mother on the same side, and both fore-paws compare with those of the mother in the same manner as those of No. 4 of the last family, the only difference being the even greater abnormality of the right paw in the present instance. The hind-paws possessed six toes with separate front pads and continuous hind pads, as in the left hind-paw of the mother. (3), also a female tabby kitten, possessed seven toes on both fore-paws. The arrangement of pads on both paws was similar to that on the left fore-paw of the mother, except that the toe B could not be said to possess a front pad at all. The hind-paws were as in the last kitten and the left hind-paw of the mother.

The next family (of three) was born in September 1885. (1), a female tabby and slight tortoiseshell kitten, possessed the normal number of five toes on the fore-paws, but the foot appeared almost as broad as in the abnormal kittens. This was because the large extra toe (A in Figs. 1 and 2) was present while the much smaller pollex 1 was absent. The front pad of the large abnormal toe was also slightly bifid, so that there was some indication of the next small toe B. The hind-paws possessed five toes with separate front pads and fused hind pads. (2), a female tortoiseshell and tabby kitten, possessed fore-paws like those of the kitten just described. The right hind-paw was also similar, with five toes, but the left possessed six like the mother. The front pads were separate, as usual, on the hind-paws. (3), a female tabby and slight tortoiseshell kitten, with fore-paws having seven toes like the mother, and also resembling her in the difference between right and left. The right paw possessed most abnormality, and was more advanced than the mother, as all the toes—even that marked B—possessed separate front pads. On the left side, however, the toe marked B possessed no separate pad. The hind-paws were like those of the mother, possessing six toes with separate front pads. This kitten was given to a friend, and will be again referred to.

The next and last family (of four kittens) up to the present time was born about July 1, 1886. (1), a female tabby kitten, was normal. (2), a female tabby kitten, possessed five toes on the fore-paws, but the feet were very broad, because the large abnormal toe (marked A, Figs. 1 and 2) was present instead of the small pollex. The hind-paws possessed six toes like those of the mother. (3), a male sandy kitten, possessed seven toes on the left fore-paw, the innermost (pollex) being *exceedingly* small and rudimentary, while the right paw possessed only six toes, the pollex being absent, although both abnormal toes (A and B, Figs. 1 and 2) were present. In this kitten the difference between the sides is therefore the reverse of that in the mother. The hind-paws possessed six toes like those of the mother. (4), a male tabby kitten,—by far the most abnormal form which has yet come under my personal notice. Both fore-paws have seven toes, each possessing a separate front pad, while the claw of the small toe B is well formed and large, and its pad is large and quite distinct and separate from that of A. The claw of the pollex 1 on both sides is partially divided (towards the apex) into a large upper, and rather smaller lower, division. This tendency towards a vertical proliferation has been already described in one of the kittens of the family born September 22, 1884. In the hind pads this was also the most abnormal form yet seen, for, interior to the normal fused hind pads for the four normal toes 2, 3, 4, and 5, were arranged three pads forming an almost continuous series with each other and with those belonging to the four normal toes. These three pads diminished in size from within outwards, and the one behind the toe B was very small, and was somewhat separated from the others, and especially associated with the internal side of the fused normal pads. The hind pads for the toes 1 and A were fused, but a distinct furrow indicated the line of separation. There was no practical difference between the fore-paws of the right and left side. The right hind-paw possessed *seven* toes, or three more than in the normal animal. This is the first time that I have come across so great an abnormality in the hind-paws, although Mr. Vaughan remembers it on both right and left sides in two individuals. All the seven toes are large and distinct, and have separate front pads. Interior to the normal fused hind pads, and continuous with them, is an ill-defined series of three pads, irregularly diminishing in size towards the interior, and crowded together so that the innermost is not behind the innermost toe. The foot is somewhat deformed. The left hind-paw possesses the usual six toes with separate front pads and fused hind pads.

I now return to (3) of the family mentioned before the last—the highly abnormal female tabby which was given to a friend in Oxford. This cat produced a family (of four) on July 10, 1886. (1) and (2), both sandy male kittens, were normal; (3) and (4), both tabby female kittens, were like the mother, possessing seven toes on the fore-paws and six toes on the hind-paws. These two kittens were given to Prof. Meldola and Mr. W. White, and I trust that they will be frequently referred to in some future number of NATURE. I am now able to give a somewhat longer account of these two kittens. In Prof. Meldola's kitten the left fore-paw is somewhat less abnormal than the right, because the toe B is very small, although it possesses a front pad separate from that of A. Of course the pollex 1 has a distinct front pad. There is a single, although somewhat divided, hind pad for the three inner toes, separate from the normal pad behind the four outer digits. On the right side the toe B is large, but the arrangement of front and hind pads is the same as that on the left side. The hind-paws have large and distinct front pads on all the six toes of both sides, and the hind pads of the abnormal toes form a continuous series with those behind the normal digits.

The fore-paws of Mr. White's kitten are precisely similar in every respect, the toe B being much larger on the right side, and the arrangement of pads being exactly the same. The hind-paws only differ in the fused hind pads for the abnormal toes being somewhat separated from those behind the normal

I. TORTOISESHELL ♀ normal (Bristol)

II. TORTOISESHELL ♀ normal (Bristol, and ? Haverfordwest)

III. TORTOISESHELL ♀ abnormal, but notes not taken (? Bristol, and Haverfordwest)

IV. TORTOISESHELL ♀ "Punch," 6 toes on all paws (Haverfordwest)

V. TABBY ♀ 6 toes on all feet (born 1877 and sent to Reading)

Very many families, among which two kittens with 7 toes on all paws have occurred. All varieties between the extreme and normal form have occurred constantly. In old age all kittens are described as normal

VI. (1) 1880 (Reading)
Tabbies
- i. ♂ FP 5 but abnormal, HP 5
- ii. and iii. ♀ normal
- iv. TABBY ♀ FP 5 but abnormal, HP 6 (Reading)

(2) 1881 (Reading)
Tabbies
- i. and ii. ♂ ♀ normal
- iii. ♀ 6 on all paws

(3) 1881 (Reading)
Colour unnoted
- i. and ii. ♀ 6 on all paws
- iii. sex unnoted, 6 on all paws

Many other families born in Reading, and always a large proportion of abnormality, but I was unable to obtain notes

VII. (1) 1881 (Reading)
Colour unnoted
- i. and ii. ♂ ♀ normal
- iii. ♀ FP 5 but abnormal, HP 6
- iv. ♀ FP 5 but abnormal, HP 5

1882 (Reading, and sent to Oxford same year)
TABBY ♀ FP 7, HP 6 (sent to Oxford)
(Others in same family unnoted)

Many other families born in Reading, and always a large proportion of abnormality, but I was unable to obtain notes

VIII. (1) 1883 (Oxford)
Tabbies
- i. ii. and iii. all ♂ and all FP 7, HP 6
- iv. ♂ RFP 7, LFP 6, HP 6

(2) 1884 (Oxford)
Tabbies
- i. and ii. ♂ ♀ normal
- iii. ♀ FP 6, HP 6
- iv. ♀ RFP 7, LFP 6, HP 6

(3) 1884 (Oxford)
Tabbies
- i. ♀ normal
- ii. ♀ RFP 7, LFP 6, HP 6
- iii. ♀ FP 7, HP 6

(4) 1885 (Oxford)
Tabbies with some tortoiseshell
- i. ♀ FP 5 but abnormal, HP 5
- ii. ♀ FP 5 but abnormal, RHP 5, LFP 6
- iii. TABBY ♀ (with slight tortoiseshell), FP 7, HP 6

(5) 1886 (Oxford)
Tabbies
- i. ♀ normal
- ii. ♀ FP 5 but abnormal, HP 6

Sandy iii. ♂ LFP 7, RFP 6, HP 6
Tabby iv. ♂ FP 7, RHP 7, LHP 6

IX. ... ...

(1) 1886 (Oxford)
Sandy i. and ii. ♂ normal
Tabbies iii. and iv. ♀ FP 7, HP 6

Capitals used for those female cats which have produced families of which some note has been taken
"I., II.," &c. = generations
"1, 2," &c. = families
"i., ii.," &c. = kittens in each family
Dates and localities indicate times and places of birth of the families to which they are affixed
"FP" = fore-paw } the numbers after = toes
"HP" = hind-paw } the letters R or L before = right or left side
"FP 5 but abnormal" indicates that the small pollex is absent, but the large abnormal toe A in Figs. 1 and 2 is in its place, making the paw abnormally broad, although with only the normal number of toes

digits on the left side, while the two sets are continuous on the right paw, as in Prof. Meldola's kitten.

All the observations recorded in this paper were made in Oxford. The abnormality has now been observed through nine generations, and I have recorded notes of ten families, so that now there is sufficient material to present in a tabular form.

The notes given in this paper are much more complete than before, because the families were born in my own house or in that of a friend living near, who kindly gave me every opportunity of making notes. The results, however, would have been far more extensive if I had received intelligence of the birth of families in various quarters to which kittens had been sent.

I believe there is little doubt that the next period of three years will produce much better results in this way, for at the recent meeting of the British Association at Birmingham I exhibited the cats, and was able to give away three abnormal females to scientific gentlemen (Prof. Haddon, Prof. Meldola, and Mr. W. White) who I am sure will assist me by sending complete accounts of all the families born. I remarked in my last paper on the immense strength of heredity which was shown in the observations then recorded, remembering that the results were in all cases due to the mothers of the families. The continued observations now published serve to illustrate the same facts. As I said before, "it is practically certain that the fathers of the families have always been normal." There has, indeed, been an abnormal male cat in Oxford for the last two years—one of my kittens which I gave to Prof. Moseley for a museum specimen, and which has been kept in order that it may be quite mature. But this cat lives at some distance from my house and that of the friend to whom I gave the female kitten in 1885, and it has never been seen in our neighbourhood, while numbers of normal cats have been seen in company with our abnormal females. But nevertheless a family containing abnormal kittens was born in a house near that in which Prof. Moseley's cat is being kept, and of which, of course, the latter must be the male parent. Unfortunately, as in so many other cases, I was unable to obtain any data, and the kittens are, I believe, all dead.

We therefore see in these observations a proof of the extraordinary ease with which a distinct breed can be produced from a spontaneously appearing variety. In spite of all the swamping effect of continual and uninterrupted crossing with the normal form, I have never been able to record a normal family, while in many cases some of the kittens were equal to, or even beyond, the abnormal parent in her peculiarity. This being the case, it is clear that a breed would have been quickly established if abnormal males had been selected to pair with the abnormal females. These observations have, therefore, an interesting bearing upon the existence of such a local breed as the tailless Manx cats, as Prof. E. Ray Lankester pointed out to me when talking over the subject. Prof. Lankester supposes that a tailless individual appeared spontaneously, and that it was considered interesting and a curiosity; and when the abnormality re-appeared in some of the offspring, these were kept in preference to the normal forms. It seems quite certain that the result might have been produced in this way, and I have arranged with Dr. Grabham, of Madeira, that some of my abnormal kittens shall be sent to him to turn loose upon some neighbouring Atlantic rock on which rabbits are the only other living mammals. I should add that Prof. Lankester found a support for the theory of the origin of the Manx breed of cats in the fact that there are tailless breeds of other animals which are also fashionable in the locality, and which seem to point to the existence of the same peculiarities of taste working upon a spontaneous variety. In fact, as Prof. Lankester suggested, the people may have rather looked out for other tailless or abnormally short-tailed animals, when their interest had been excited by the existence of one such breed. But the observations here recorded have also a bearing upon those cases in which natural, instead of artificial, selection has been the agent. Granting, as I believe we must do, that some adaptive characters of great importance owe their beginning to flashes of structural or functional originality—appearing suddenly and spontaneously in one individual, as the extra digits appeared in the ancestor of my cats,—we see from these observations that in spite of all the effects of constant intercrossing with normal forms, there would be a most persistent offer of material upon which natural selection might work, for the variation would appear to a greater or less extent in a very large proportion of the individuals of the various families produced, while again and again the peculiarity would be inherited in a form equal to or even beyond that of the parent. It is therefore of interest to actually test a few instances in as complete a manner as possible, taking care that only one parent possesses the abnormality, for this is what must have happened for the first few generations of any such variety which originally appeared in a single individual in a natural state. It is chiefly with the object of adding another to the instances already known and worked out that these observations have been undertaken, and will be continued and rendered as complete as possible. It need scarcely be pointed out that such instances differ essentially from all the cases in which breeds of domestic animals have been established, for in these well-known and numerous breeds heredity has had undisturbed possession of the field, without any confliction between the normal and abnormal forms, except indeed in the case of the first family produced by the original parent of certain breeds of which the peculiarity appeared spontaneously in a single individual, as in the breed of "otter" sheep.

EDWARD B. POULTON

LIGHTHOUSE ILLUMINANTS[1]

THE details of the construction of the three towers and lanterns, and of the lenses and lamps in each lantern, of the magneto-electric machines, and of the gas-works, have no doubt been placed on record, and will be reported by the Trinity House engineers. But the following may serve as a general description of the arrangements.

Three low towers, constructed of massive timber, have been erected in a line inland from the higher of the two permanent lighthouses on the South Foreland, the nearest being 245 feet distant from the lighthouse, and the three being separated one from another by intervals of 180 feet. Their height, varying with the level of the ground, so that the lanterns may be on the same level, is from 20 to 30 feet; upon these structures rest three similar lanterns about 20 feet in height and 14 feet across. Within the lanterns are columns of lenses forming two opposite sides of a hexagonal framework which rises from the base to near the top of each lantern. The whole framework can be made to revolve so that either column of lenses may be made to face in any direction; each column consists of three or four similar lenses superposed, but the lenses forming different columns are different in their purpose and structure, and in their size. One column in each lantern consists of lenses designed to gather the divergent rays which fall upon them from the central source of light into a level sheet which spreads over the surface of sea or land, but not downwards or upwards; each of these lenses is a segment of a cylinder, and may be described as a cylindrical lens. The opposite column in the gas and oil lanterns consists of lenses designed to gather the divergent rays, not into a sheet, but into a single cluster or cone of small vertical angle, which is sent forth horizontally in any one direction. These lenses are made up of a central circular lens, surrounded by annular prisms and segments of such prisms, the whole fitting into a rectangular frame; they may be called annular lenses. The corresponding column of lenses used with the electric light consists of cylindrical lenses with condensing prisms placed in front of them; the cylindrical lens flattens a broad cone of light into a fan, the condensing prisms close the fan.

The size of the cylindrical lenses placed in front of the gas and oil lamps is the same, but the lenses in front of the superposed electric lights are smaller. The annular lenses, of which three form a column in the oil lantern, are each 6 feet 3 inches in height, while the four superposed annular lenses in the gas lantern are each 3 feet 9 inches in height. Both sets of annular lenses have the same width, namely, 3 feet 5 inches.

The electric lights are large arc lights, supplied with the electric current by three magneto-electric machines, which are worked by the steam-engine in the engine-house built for the ordinary work of the station. The electrical apparatus is of the construction of Baron de Méritens.

The gas-burners tried hitherto are of Mr. Wigham's construction, consisting each of a multitude of small fish-tail jets on brass stems about 6 inches long and an inch one from another, arranged on the same level in concentric rings. A tall funnel, a few inches above the cluster of burners, draws their flames together into the form of a bell. The number of concentric rings may be changed quickly so as to increase or reduce the size of the burner from a diameter of about 4 inches with 28 jets, to a maximum of 11 inches diameter and 108 jets.

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[1] Preliminary Report of Mr. Vernon Harcourt to the Board of Trade on the Experimental Lights exhibited at the South Foreland.

The oil lamps are of the usual Trinity House pattern with six concentric wicks, and are fed with paraffin oil.

Cannel gas is manufactured and stored at a short distance from the experimental towers, and supplied through a meter to the gas-burners.

For the observation of the lights, which were first shown in the week beginning March 30, three huts have been erected at different distances along a line perpendicular to the line of the towers, and this line has been marked by posts showing the distance from the central tower. The lighthouse-keepers who are stationed in one or other of these huts are instructed to make hourly observations during the time the lights are exhibited, expressing in figures their estimate of the relative brightness of the three lights. When the night is misty the keepers are instructed to patrol the line of posts, and to record the distance at which each light is lost or becomes visible. To avoid prejudice in favour of either an old or a new mode of lighting, the towers have been labelled, and are called A (electricity), B (gas), and C (oil). The huts are numbered. No. 1 is rather more than 700 yards distant from the central tower, No. 2, about 1¼ mile, and No. 3, 2¼ miles. Steps have also been taken to obtain estimates of the relative brightness of the three lights from observers at greater distances. To secure the identification in each hut of the lights observed when all three are not visible, three tubes have been fixed in each hut directed towards the lights A, B, and C, and labelled accordingly.

The huts serve also for measurements by various photometric methods of the light sent forth from each lantern. A number of such measurements have already been made, the results of which have been communicated to the Board of Trade by the Trinity House.

Near the engine-house on the South Foreland a long gallery has been built, in which the light emitted by the various lamps employed or proposed to be employed, can be measured so as to ascertain the value of these lamps independently of the lenses by which, within the lighthouse lanterns, their apparent brightness is variously augmented.

The experimental inquiry thus instituted will serve :—

(1) To ascertain the amount of light given by the six-wick and seven-wick oil-lamps, and of other oil-lamps, or modifications of them (if any) which may be proposed for lighthouse service.

(2) To ascertain similarly the amount of light given by Wigham's gas-burners on different scales (28, 48, 68, &c.) with different rates of consumption, and, if thought well, with different qualities of gas, and to test other gas-burners in like manner.

(3) To furnish further and trustworthy measurements of the light given by the electric arc with various carbons and with various tensions and quantities of electricity, and to test the efficiency of the De Méritens magneto-electric machines in converting mechanical into electrical energy, and whether they work without difficulty or risk of break-down or need of repair or loss of power; also to test the working of the De Méritens electric lamp, and of other electric lamps, if thought well.

(4) To furnish additional data for estimating the cost of maintaining any given light for a certain time, say 1000 candles for one hour, by each mode of producing light, and on the various scales suitable to different localities.

(5) To measure the efficiency of the lenses employed, especially with flames of different sizes in their foci.

(6) To prove experimentally (if such proof be desired) that 2 or 3 or n similar lights, when juxtaposed, give twice or thrice or n times as much light as a single light gives.

(7) To ascertain what light is sufficient to be visible from its horizon on a clear night, and in what ratio on the average of many nights the visibility of a light at great distances increases with its total intensity, or lens area, or proportion of red or of blue rays.

(8) To test the effect of the variations last named in haze, or mist, or fog, or rain, or snow, that is, when the air is made more or less opaque by particles of liquid or solid water of various sizes suspended in or falling through it. Such testing may be made either photometrically, which is only possible in slight haze and at small distances, or by observations of the distance at which each light is lost or reappears.

(9) To try the question of the utility of ex-focal light, whether, that is, it often happens that the position of a lighthouse may be seen by the illumination of cloud or fog above or around it, when its position would be unknown if equal light from a smaller focus were directed almost wholly towards the mariner, and not allowed to spread.

(10) To test further whether in mist or haze sudden flashes of a powerful beam of light are noticeable when an equal light maintained constantly, or waxing and waning gradually, would not be noticed.

It is likely that other subjects of experimental inquiry may be suggested by those experienced in lighthouse illumination, or may occur as the experiments proceed. But, taking those above enumerated in order, I will attempt to indicate the conclusions which at present appear probable, and to make some suggestions as to points still to be investigated.

(1) It appears that the six-wick oil lamp behind the annular lens sheds light of as great intensity as the seven-wick lamp, while its consumption of oil is much smaller. Probably this result is due, in part, to the fact that the outer ring of flame which the seventh wick adds is further from the focus of the lens, while each ring of flame is partially opaque to the light from the rings inside it; and partly to the fact that the seven-wick lamp has not yet been brought to so perfect an adjustment of oil-supply to air-supply as the six-wick lamp.

I do not know whether any oil-lamp used in other than English lighthouses is such as to merit a trial against the Trinity House lamp.

(2) Some observations have been made with Mr. Wigham's burners with 88 and with 108 jets, which seem to show that with gas as with oil, behind the annular lens, no gain in intensity of light results from the circaposition of another ring of flame. Some evenings should, I think, be devoted to trying this question out. The value of ex-focal light behind an annular lens seems to be almost *nil* as regards intensity, and, if so, it may be well to use with revolving light a smaller flame than that of the six-wick lamp. Excellent experiments on this question can be made with Mr. Wigham's burner by exhibiting on a clear night through the annular lens one of these burners, whose size should be reduced, after an interval sufficient for photometry, from 108 jets to 88, and so on to the smallest size, measuring also after each change the consumption of gas. It will probably be found that a large fraction of the *directed* light is still obtained with a relatively small consumption of gas, and with the accompanying advantage of a low temperature within the lantern.

Similar measurements should be made with a cylindrical lens and with the naked flame in the photometric shed.

At present one other gas-burner besides Mr. Wigham's has been tried, a ten-ring gas-burner devised by Sir J. Douglass, which has given an excellent yield of light. Two others, by W. Sugg and Co., and by the F. Siemens Company, await a trial. The problem which the maker has to solve is to pack as much highly luminous flame as possible into a sphere of 3 or 4 inches diameter.

Where gas has to be manufactured expressly for a lighthouse, it would generally be best to make cannel gas, but near a town where common gas could easily be laid on, it would be cheaper to use common gas. It might, therefore, be worth while during the course of the experiments to charge the small gas-holder with common gas, and to note the consumption and the light developed. It would probably be found that with suitable burners the chief disadvantage in using common gas was the greater development of heat, the same light being obtained from the consumption of a larger volume of lower priced gas.

(3) Many measurements have been made in recent years of the light of the electric arc, but the difficulty of making measurements of so variable a light, and the uncertainty attaching to the standards of light employed, and the great differences between one arc light and another, according to the electric current and the carbons employed, make it clearly desirable to have further measurements of the electric light at the South Foreland.

Photometry should be accompanied, as with oil and gas, by a measurement of consumption. The mechanical energy absorbed can be measured at the strap which connects the magneto-electric machine with the steam-engine. The electrical energy developed can be measured in tension maintained, and quantity used, at the leads connecting the machine with lantern A. The cost of each horse-power per hour on the actual scale of working at the South Foreland must be already known. The rate at which the two forms of carbons which have been tried are consumed is also known.

It is essential to the value and significance of the photometry that simultaneous electrical measurements should be made.

The possible variations in the coupling of the magneto-electric machines, in the rate of running, and in the nature, form, and adjustment of the carbons, present a wide field of experimenting.

The continuance of the experimental working for many months will serve for a trial of the trustworthiness of the De Mériten's apparatus for lighthouse service.

(4) The cost of maintaining a lighthouse supplied with gas has been very variously estimated. It must vary from place to place, especially with the price of coal. The actual working expenses of oil lighthouses on the English coast, and gas lighthouses on the Irish coast, with allowance for the price of coal and labour, should furnish trustworthy data for a comparison. But to complete these data the quantity of light produced and utilised in each case needs also to be known, and as both the oil and gas burners tried hitherto at the South Foreland are of the service kind, the photometry now in progress will supply this knowledge. Some information may also be gathered from the expenditure on each illuminant at the outset and during the course of the experiment. It should be possible to state, if it were desired to maintain on the South Foreland a light of 50 or 100 or 200 thousand candles, what its annual cost cost would be with each illuminant.

(5) Although the action of lenses is mainly calculable, and, so far, does not require trial, it is modified by two quantities which vary slightly, namely, the reflection and absorption of light by glass, and is affected to some extent by errors of workmanship. It will, therefore, be of interest to obtain an exact comparison between the light emitted by a naked flame, and that from the same flame concentrated by different types of lighthouse lens. The prediction of the effect of a lens is less possible when the illuminant is of large size; and the failure of lenses, constructed for use as a revolving light with gas, to utilise (except by broadening the beam) the light produced at a distance of more than two or three inches from the focus of the lens, if they are found to fail so far, may be worth demonstrating.

With the electric light a very close correspondence should be found between the calculated effect of the cylindrical lens and of the condensing prisms and the results of photometry.

(6) The measurements which have been made of multiform gas and oil may be taken to show that any number of lights at a given distance cause so many times the illumination which one light causes; or assuming that the above must be the case, and is involved in the conception of comparative illumination, the proportional variation of the photometric results with the changes from uniform to biform, &c., on clear nights, gives evidence of the trustworthiness of the photometric methods.

(7 and 8) The two questions, which I have numbered thus, can hardly be treated separately, since clearness differs only in degree from slight haze, and slight haze from fog. Together they constitute the chief object of this inquiry.

The observations of the experimental lights which have been made from a distance, may be expected to yield, when they are collected and compared, much information as to the distances at which the several lights have been seen in various weather. But the changes which are necessary when photometric testings are to be made, or when an extensive programme is to be exhibited, must to some extent have interfered with the observation of the changes due to variations in the transparency of the atmosphere. It might be well for at least one month, to show the same lights nightly, and to inform the distant observers that this was about to be done, in order that their observations might have the more value. A single light of each kind shown through the cylindrical lenses would serve as well as multiform lights, and it would be best to use that size of gas-burner which had through the lens equal illuminating power with the six-wick oil lamp. Unless, indeed, it is assumed, as I should be inclined to assume, that equal lights from gas and oil have the same power of penetrating haze; in which case it would be more instructive to show from tower A a single electric light supplied from one machine, and from B and C either gas or oil also single, and either oil or gas of such size and number as to have at close quarters on a clear night an illuminating power equal to that of the electric light. A sufficient series of distant observations of these lights would show (1) whether the electric light maintained its equality with the larger hydrocarbon flame through slight haze, or became more nearly equal to a flame of much less initial brightness; and (2) whether the taller beam of multiform oil or gas had much advantage over the beam sent forth from a single lens. I believe it will be found that the relative brightness of two, or more, to one, will be maintained at any distance and through any haze which permits of photometry, but that, when the single light is lost at 5 miles or 500 yards, the triple light will be invisible at 6. The actual figures corresponding to these conjectural figures must be found, and the Trinity House Committee will then be able to judge in what cases such an extension of range is worth the increased expenditure.

In the case of the electric light, the observations which have already been made show that it loses in haze a larger proportion than the hydrocarbon flames. Further observations on this point will be of much interest and importance. The most valuable are observations of the distances at which an electric and a gas or oil light, whose relation in clear weather is known, cease to be visible. Such observations are strictly photometric observations, in which the lights observed are brought to an equality of minimum appreciable brightness, and the distances at which their brightness is equal are measured. These are dependent upon the weather, and may be practicable on only a few days in each month. Still more rarely will the opportunity offer of measuring the lights in hut No. 1 through a slight uniform mist; but such measurements ought to be made. I would suggest the possibility of testing in the photometric shed through an artificial mist produced by blowing steam from the boiler in the adjoining engine-house into the middle of the shed.

It is said that Faraday proposed at first the use of a very small lens with the electric light. Unless conclusive experiments have been made on this point, it may be well to place the experimental electric light in the focus of a larger and of a smaller annular lens, each subtending the same angle, and to note whether the effect differs.

It might also be worth trying whether biform gas, with a small enough number of jets to have the same illuminating power as single oil, would be better seen through slight haze. The trial would not be between gas and oil, but between placing a strong light behind one lens, and placing half the light behind each of two superposed lenses.

(9) Owing to the nearness of the three lanterns, the illumination or halo which spreads round each of them in a fog seems almost to blend. That which surrounds the gas lantern is not much greater than that around its neighbours on either side. I do not think that, on the one occasion on which I have seen the lights in a fog, the ex-focal light was of much service.

If a lighthouse lantern was surrounded by a mist or cloud extending far enough laterally to extinguish its principal beam, but so little above it as to allow the scattered light to fall upon a higher stratum of cloud, the position of the lighthouse might only be seen from the illumination of the cloud above it. But this state of things would happen rarely in most places, and a better plan of turning it to account than the addition to a burner of rings of ex-focal flame would be to employ the upper prisms to send a second beam skyward. Whether the general illumination about the experimental lanterns has been visible when the three centres of light were not visible to an observer towards whom the beams were directed, may perhaps be gathered from the record of observations.

(10) When engaged on a clear night in judging of the experimental lights, the eye of the observer is continually caught by the sudden flashes of the Calais light. The revolving light at Grisnez is equally visible, but does not catch the eye in the same manner. It might be well to try on some rather hazy night, whether, if one lantern alone were lighted, and during successive quarters of an hour the light were alternately kept steady and flashed in some such groups of flashes as the Calais light, the observers patrolling the line of posts became aware of the light at a greater distance when it was flashed than when it was steady or revolving. Even a slight mist is a great leveller of distinctions, but it seems possible that the use of flashing may increase the range of a light as much as an addition to its intensity or size.

Some of the questions raised in the latter part of this report might perhaps have been omitted, as having already received an answer, if, while thinking the matter over, I had been able to consult some of the experienced members of the Trinity House Committee who are charged with the conduct of this inquiry. I have ventured here in writing, as at other times by word of mouth, to make the suggestions which have occurred to me, knowing that they will receive friendly attention if they are submitted to the Committee, and hoping that some of them may be of service. (Signed)

July 26, 1884 A. VERNON HARCOURT

Since[1] the foregoing preliminary report was presented to the Board of Trade, the experimental inquiry has come to an end, and a complete account of the apparatus, observations, and testings has been published by the Committee of the Trinity House who had charge of the inquiry, followed by a statement of the conclusions at which the Committee have arrived.

I propose to arrange the remarks I have to offer under the following heads:—

I.—Apparatus for the exhibition of the experimental lights.
II.—Arrangements for observation.
III.—Photometry.
IV.—Comparison of lights.
V.—Range of lights in hazy weather.
VI.—Cost of each system.

I.—*Apparatus for the Exhibition of Experimental Lights*

In my preliminary report I have given a general description of the temporary towers, the lenses and lamps. In Parts I. and II. of the Trinity House Report are to be found plans and measurements giving the full details of these constructions. The towers are admirably suited to their purpose, and their situation and the distance between them proved most convenient for observation.

In regard to the arrangements for exhibiting the electric light, it is to be observed that, although the electric light completely outshone its competitors, it was heavily handicapped in the competition. The "leads" were not of sufficient calibre to carry the large electrical currents used, for a distance of nearly 300 yards, without considerable loss. Prof. Adams estimates the loss at more than one-fourth the electrical energy supplied.

The five vertical prisms used in the case of the electric arc to bring together the horizontal rays, subtended an angle of only 30°, while the annular lenses which served the same purpose in the gas and oil lanterns subtended an angle of 60°. Thus the fraction of the light emitted from the central source of light, which composed the revolving beam, was only half as great in the case of the electric arc, as in the case of the gas and oil flames. It seems probable also that a beam of less divergence may be used with advantage to obtain a maximum range in hazy weather; and such a beam may be obtained from the electric arc with lenses of moderate size. On a few occasions when an annular lens similar to those used in the other two lanterns was placed in front of the electric arc, the light was dazzling at a distance of more than a mile, and surprisingly vivid at a distance of 20 miles. I see that on a clear night when the 108-jet gas burner behind an annular lens gave a light of 60,000 candles, the electric arc behind its cylindrical lens and vertical prisms gave a light of 1,200,000 candles, and behind an annular lens a light of 12,000,000 candles. In the one case the arc was five times as powerful as Mr. Wigham's "quadriform," in the other fifty times as powerful.

For the sake of uniformity and comparison under similar conditions, only the central belt of the Fresnel apparatus was placed round the electric lamp as round the gas and oil burners. The suppression of the top and bottom prisms, though entailing a loss of 30 per cent. of the light produced, is a necessary sacrifice where large burners developing great heat are placed immediately one over the other. But each of the electric lamps in tower A might have been surrounded with a complete Fresnel apparatus, adding nearly one-third to their light, without any difficulty or any necessity for separating them more widely.

Thus, if the principle which has been enunciated had been followed, of doing for each illuminant the best that could be done within the limits of the lighthouse lantern, if a triform electric light had been exhibited, with leads of low resistance, with a lens subtending an angle of 60°, and with top and bottom prisms, the power of this light might have been more than tripled. By also reducing the divergence of the beam, which I think might be done with advantage, a further increase of power could have been gained. This fact should be borne in mind in comparing the results which were obtained with the three illuminants.

In M. Allard's interesting and important "Mémoire sur les Phares électriques," 1880, he gives the results of a trial of three Gramme dynamo-machines and an electro-magnetic machine of the Alliance Company. The former gave for the same horse-power 40 or 45 per cent. more light than the latter. But M. Allard measured only horse-power and light, not the electrical energy developed; and it does not appear whether the larger yield of light was due to a more powerful electrical current, or to the position of the carbons, and the form of the incandescent ends, being more favourable to the emission of light with the continuous current. Probably the De Méritens machines, which produced a light of about 1000 candles per horse-power, are superior to those of the Alliance Company, which yielded only 540 candles, and are equal to the Gramme machines which yielded 800 candles per horse-power. Of all that relates to the economical production of powerful arc lights, knowledge is advancing rapidly. The ample provision of steam power, and the excellent photometric gallery at the South Foreland, will no doubt be used from time to time for the trial of new types of electrical machines, of regulators, and of carbons. For the past experiment, and apart from the question of cost, the De Méritens machines worked admirably, converting, according to the measurements of Prof. Adams, mechanical into electrical energy with a loss of only 16 per cent. The current supplied was more than sufficient for the largest carbons; indeed, carbons exceeding 1½ inches in diameter were heated to redness through their entire length.

In regard to the apparatus for exhibiting the gas system of Mr. Wigham and the oil lamps of the Trinity House, little can be added to the full and clear account of the Trinity House Committee. But as it has been stated, since the publication of the Trinity House Report, that Mr. Wigham's foreman was not left unfettered to make the best display which the apparatus in his charge would allow, I may here put on record what I saw and believe in the matter. I paid many visits to the gas lighthouse by day and by night, and was in frequent communication with the foreman, Mr. Higginbotham, from the beginning to the close of the experiments. The arrangement of each night's programme rested with the Committee of the Trinity House, who so ordered matters that abundant opportunity was given for the observation and measurement of all the varieties of each illuminant. Among these were Mr. Wigham's combinations of 28 jets, 48 jets, 68 jets, 88 jets, and 108 jets, the ready conversion of one of which into another is among the merits of his ingeniously constructed burner. When the effect of the smaller number of jets was to be observed, it is clear that the full power of the burner could not also be shown. Therefore, there were necessarily times when Mr. Wigham's foreman was not free to make the best display which the apparatus in his charge would allow. With this exception only, I believe that Mr. Wigham's foreman was perfectly free to do his best and make any improvements Mr. Wigham or he could devise. I see from the summary in the Trinity House Report that the full power of Mr. Wigham's burners was shown on 127 nights; and it appears from the photometric record that it was measured 57 times. This ought to suffice for an accurate judgment of its merits.

Comparing the gas and oil towers as they appeared to a visitor when in full operation, the gas had one striking advantage, and one equally obvious disadvantage. The advantage was that it needed no care. When the lenses had not to be revolved by hand, nor the number of jets changed, one attendant in the tower was sufficient, and he had little or nothing to do. In the oil tower, on the other hand, I have seen a keeper on every one of the three stages, each man watching and from time to time adjusting his lamp. The disadvantage encountered in the gas tower was the excessive heat from the large gas-burners, which by causing unequal expansion of the glass lenses and their metal framing, and of the outer and inner surfaces of the lenses themselves, caused cracks to appear, which in the continuous belt of thick glass gradually spread from side to side. But though the burning of gas yields for the same light more heat than the burning of oil, there is no reason to think that with a diminished consumption of gas, *e.g.* the 160 cubic feet an hour of the 68 jets instead of the 300 of the 108 jets, such a disaster would recur. When the gas flame is surrounded by a chimney, as in Sir James Douglass's and Mr. Sugg's multiple Argands, the heating of the lenses is greatly diminished.

When the lights were first exhibited, the behaviour of the oil lamps in C tower was a matter of much interest. Using gas, Mr. Wigham had succeeded in quadrupling the power of a large burner behind a lens more than a yard square, by placing over it three other similar burners and lenses. But it seemed a hazardous experiment to imitate this plan by placing three lamps fed with mineral oil one over the other. However, the skilful arrangements of Sir James Douglass were completely successful. The three superposed oil lamps burnt as safely and well as if each had had the lantern to itself.

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[1] Further Report of Mr. Vernon Harcourt to the Board of Trade on the Experimental Lights exhibited at the South Foreland.

II.—*Arrangements for Observation*

A short account of these arrangements have been given in my previous report, and a complete account is to be found in the report of the Trinity House Committee.

The plans for making observations on shore at small distances had been well laid. The home at St. Margaret's, stationed between the two observing huts, with telephone to all points; the measured distances; the huts themselves, welcome refuges on a cold night, and most convenient for photometry with their helpful occupants; all bore witness to the wise forethought which had been bestowed upon the details of the inquiry.

For obtaining records of the relative brightness of the different lights from the impressions of those who saw them, probably no better plan could have been devised than that of distributing forms to be filled in with a numerical estimate of the ratio which two of the lights bore to the third. And the enlisting a multitude of observers, by the wide distribution of these forms, secured the two advantages, of an average drawn from a very large number of observations, and of an obviously impartial judgment. The observations made at sea from the Trinity House yacht *Argus*, which was in constant attendance, were of great importance; and I may add that, for the landsmen whose main business was photometry at small distances on shore, taking part in these observations was an essential help towards the full appreciation of the problem before them.

III.—*Photometry*

The chief assistance which I found myself able to render to the Committee was in devising and improving photometric apparatus and methods. A full description of these is given in the Committee's Report, especially in Mr. Dixon's "Record," part ii., pp. 30-36. During my visits to the South Foreland, I was principally occupied with photometry, in the dark gallery by day, and in one of the huts by night. Frequently Mr. Longford or Mr. Dixon worked with me, and the observations which I made are included in the general record. I believe that the standard of light employed was constant and of a definite and reproducible value, and that the methods of comparison were trustworthy and accurate. The excellent idea of Sir James Douglass, of using a large lens to concentrate the rays from the lighthouses upon the photometric disk, made possible the measurement in the more distant hut of lights whose intensity was too feeble to be accurately estimated without such aid. Mr. Dixon's polariscope photometer and the ingenious obscuration photometer of Captain Nisbet, are instruments well adapted for the direct comparison of distant lights or lights enfeebled by haze; the former can only be used for lights which are near together. Two movable photometer-bars, designed by Sir James Douglass, and suitable for use with any form of disk and any standard, were in constant employment throughout the trial. These were placed in the photometric gallery and in hut II. The observations in hut I. were reduced by means of a portable bar devised and made by Mr. Dixon.

A glance at the 16 columns of the closely-printed photometric record,—each number being, as a rule, the average of many observations,—will give to those who know the effort of attention which accurate photometry requires a conception of the diligence with which this branch of the inquiry was pursued. I have spent many hours in one or other of the huts with Mr. Dixon or Mr. Longford, and I wish to express my conviction that the results which they obtained and which are printed in the Trinity House Report, are as complete and trustworthy as zealous, patient, and skilful work could make them.

IV.—*Comparison of Lights*

Although an observer's opinion on the relative brightness of two or more lights, like an opinion on the force of the wind, is better expressed by means of numbers than by descriptive terms, such numbers must not be regarded as expressing the relative intensity of the lights so compared. Perhaps on any future occasion it would be better to call the brightest light 10 rather than 100, since the use of the larger number suggests that an inferiority just sufficient to be noted with confidence is to be expressed by a difference of 2 or 3 per cent., whereas it probably amounts to at least 10 per cent., and 9/10 or 8/10 would be nearer the ratio of the two lights than 98/100 or 95/100. No doubt by practice in comparing lights whose relative intensity is known, a fair power of judging may be acquired; but without such training the natural tendency is to under-estimate differences. For example, the average of 294 estimates by eye of the relative power of "triform oil" and "quadriform gas," assigns to the gas a superiority of 6 per cent., whereas the actual superiority as shown by measurement is 23 per cent. According to the same series of estimates the electric light has a superiority over "quadriform gas" of 59 per cent., the actual superiority being more than 400 per cent.

Equally remarkable evidence of the tendency to overlook differences of intensity when the estimate is made directly by eye, is found on comparing the values assigned to "multiform" lights. The figures relating to the 108-jet gas-burners, and representing the relative value of the single, biform, triform, and quadriform lights, each by comparison with the electric light, are 56, 61, 59, and 63. If these numbers represented the intensity of the light falling upon the eye from the whole surface of the illuminated lenses, they should stand in the ratio of 1, 2, 3, and 4. The explanation at once suggests itself that while the photometer measures the total light received from a large illuminated surface, the eye judges of the brightness of the surface or the light received from equal areas. To an observer looking down a street on a clear night, the more distant gas-lights seem as bright as those which are nearer, though smaller in size; if asked to estimate the lights he would probably assign the same figure to all. And the visual angle subtended by the flame of a street-lamp at 100 yards is about the same as that subtended by 18 feet of lenses at a distance of 4 miles. The singular fact that as estimated by eye, on a separate comparison with the electric light, "multiform" have no superiority to single lights, may to a small extent admit of the explanation which applies to the familiar case which has been given. In this case the observer distinguishes between size and brightness, and sets himself to judge of the latter only; or it may be that the intensity of the sensation of light depends upon the brightness and not the size of the spot of light formed upon the retina. But the South Foreland observations were not made chiefly at distances of only 2 or 3 miles; nor were the observers likely to disregard the apparent magnitude of a light in estimating its value. At distances of from 12 to 14 miles, at which the largest lights have no appreciable magnitude, the average values assigned to the biform, triform, and quadriform lights are 75, 66, and 60.

I fear the true explanation is that the results have suffered from the electric light having been adopted as the term of comparison. To a small extent its fluctuations and difference of colour, and to a much greater extent its incomparable power, have made the estimates entirely uncertain; and thus it is vain to institute cross comparisons between the different lights which were not seen together, but only estimated by reference to the electric light as a standard. This conclusion, however, does not affect the value of the comparisons, chiefly aimed at, between the gas and oil lights, which were seen together, nor the significance of the direct comparison of the flame lights with the electric light at all distances and in all weathers. Indeed, the adoption of the electric light as the standard with which all others were to be compared, has served to establish on the basis of thousands of observations the important fact that, as far as the eye can judge, the electric light appears to excel the light of gas or oil lamps almost as much at greater as at smaller distances, and in hazy weather as in clear. The mean ratio of the electric light to all the gas and oil lights exhibited, taken from the whole number of recorded observations, is, at distances of from 1 to 8 miles 1000/626, at distances of from 8 to 15 miles 1000/613. In clear weather the mean ratio is 1000/591; in weather not clear it is 1000/608.

The photometric record presents many points of interest.

In the measurement of naked flames the long gallery, which has wisely been made a permanent structure at the South Foreland, afforded unexampled facilities. The electric arc was measured at a distance sufficiently great for its intensity to be similar to that of the other lights which were measured. The values assigned to it, from 10,000 to 15,000 candles, are not so high as some which have been obtained; but this is perhaps due to the fact that extreme values were rejected, and care was taken to obtain an average result.

In one respect the observations are incomplete, and need to be supplemented at some future time. Within the lighthouse the source of light is surrounded by an apparatus which gathers together the light sent forth in all directions, excepting a small angle above and below. Thus, the intensity of the light sent in a sloping direction upwards and downwards is of as great importance as that of the light sent forth in the horizontal plane. But only the latter has hitherto been measured in the photometric gallery. According to M. Allard the electric arc produced by an alternate current sends out horizontally an amount of light which is 11 per cent. greater than the average amount sent in all directions. With a continuous current the strongest light is thrown on the side opposite to the positive carbon; but it happens that with an arc light of this description horizontal measurement gives the average value. Probably Mr. Wigham's wide cluster of gas jets sends forth less light horizontally, owing to the imperfect transparency of one flame to the light of another, than it sends in an upward direction; and the same may be the case with the concentric gas-burners and oil lamps. Mirrors might hereafter be arranged within the photometric gallery, which would serve for making these measurements.

To determine accurately at a distance the power of the various lights exhibited was an essential preliminary to calculating the range of any of the lights in hazy weather. And although this power is approximately calculable, the power and dimensions of each flame, and the structure of each lens being known, it was of great interest to make actual measurements of the intensity of the light at two different points, and in different states of the atmosphere. I am not aware that such measurements had ever before been attempted. Owing to the novelty of the photometric problem, and to some extent of the methods employed, it was highly important to have some means of testing how far the results were trustworthy. Such means were furnished by the multiform system of Mr. Wigham. At any distance, and in any state of the atmosphere, the illumination produced by a combination of two or more similar lamps and lenses is so many times as great as the illumination produced by a single lamp and lens of the same kind. Thus, among the lights to be measured were several whose relative power was known beforehand. If testings of single and multiform lights, made in succession while the degree of clearness of the air was unchanged, gave values varying approximately as the number of lights, an equal degree of exactness may be ascribed to the testings of other lights and lenses.

On July 12, the weather being "clear, calm, overcast," the light from the single, biform, triform, and quadriform 108-jet burners, showing through Mew Island lenses, was measured at hut I. The results in thousands of candles were 50, 98, 168, and 214. Three testings intervened between the second and third, and probably the air had become a little clearer, but the numbers are not far from the ratio 1, 2, 3, 4. Three days later the same single, triform, and quadriform combinations were tested one after the other, also at hut I. The values found are 48, 145, and 186. Similar testings were made on July 23, on a clearer night, of all four combinations; their power was found to be 58, 112, 171, and 220 thousands of candles. Probable values in the ratio of 1, 2, 3, 4, are 56, 112, 168, and 224. On the same night the value found for a single six-wick oil lamp behind an Eddystone lens was 56,000 candles, and for three such lamps behind three such lenses 168,000 candles. On November 1, in thick haze, the value found at the nearer hut for "I. Gas, 108, M.," was 25,500 candles, and for "IV. Gas, 108, M.," 102,000. The results obtained at hut II., with a wholly different photometer, are confirmed in the same manner. For example, on February 7, consecutive testings on a misty night of "single" and "quadriform gas" gave in thousands of candles the numbers 26 and 101. On March 20, on a very clear night, the values found for the same two lights were 63 and 252.

In looking over the tables of the photometric record, and comparing the figures standing against combinations of equal numbers of oil lamps behind Eddystone lenses, and of the larger gas-burners behind Mew Island lenses, in all weathers in which the lights were measurable, the eye is struck by the similarity of the numbers. The rival systems are nearly equal; there is little to choose between them. Still less difference, as has been pointed out, was discernible on looking, as we did night after night, at the lights themselves. Other considerations than that of visibility in either clear or hazy whether, must decide which, if either, of the two systems is to be generally adopted for lighting our coasts. By multiplying burners and lenses, and by enlarging the size of the lenses, more powerful lights still may be produced, if it is thought desirable, with either illuminant.

But the most prominent fact on the face of the photometric record is the immense superiority of the electric light. The conclusion forces itself upon the reader of these tables that if greater power is needed, it is to be found, not by magnifying lenses or multiplying combinations of gas or oil burners, but by substituting the light of the electric arc.

The Trinity House Committee report that the electric light in clear weather is certainly not popular with sailors, chiefly on account of its dazzling effect at short ranges. But at ranges exceeding two or three miles, "hyper-radiant," or even multiform lenses, are not visibly larger than such a lens as is suitable to the electric arc; and at such ranges the "dazzling effect" is simply that due to the power of the light. If a double quadriform were as powerful it would dazzle as much. Also the use of a powerful electric arc in clear weather may be avoided. It would not be difficult to arrange for the use of a small electric arc during clear weather, and the quick substitution of a powerful arc light when the weather became hazy. I would venture to suggest that the singular circumstance which led or contributed to the removal of the electric light at Dungeness, that a vessel went ashore near the lighthouse, may have been due, not to the dazzling effect of the light, but rather to the diminution of brightness as the approaching vessel passed within and beneath the range of the light. With the condensed and sharply-defined beam of the electric light, it may perhaps be desirable to devote some part of the optical apparatus to spreading a portion of the light over the space intervening between the coast and the point, a mile or two away, at which the principal beam first strikes the sea. If this is done, the light at short ranges might be made sufficient, but not too dazzling; and for longer ranges there seems to be no reason why the powerful beam produced by the electric arc behind one of the Mew Island lenses should not be employed. This beam had a divergence of about 1°. Even from the high level of the South Foreland lights, if the axis of such a beam were so inclined that only about ⅛ of the light passed over the horizon, the full light would extend to within about three miles of the shore. Since the apparent brightness of every light must vary with the state of the atmosphere, as well as with the distance of the light, and as the angle subtended, even by a multiform light, at a few miles distance is very small, it cannot be possible by the appearance of a light without other data to judge of its distance. The electric light is not singular in this respect.

(*To be continued.*)

THE LUMBAR CURVE IN MAN AND APES

WE are indebted to Prof. Cunningham, of Trinity College, Dublin, for a well-illustrated and exhaustive memoir on the subject of the lumbar curve in man and apes. This memoir has been printed by the Royal Irish Academy as one of the Cunningham Memoirs, and is illustrated by thirteen plates, several of which are large folding ones, and two of which are large coloured drawings of the two surfaces of a mesial section of a male chimpanzee; these are life-size, and are the first accurate representations of the topographical anatomy of this anthropomorphic ape we have seen.

The structural differences between man and the anthropoid apes are no doubt in a great measure due to the assumption by man of an erect attitude, and to his having from an early period of his life dispensed with the use of his anterior extremities as organs of locomotion. The vertebral column of man might be expected to exhibit in a marked degree differences distinguishing it from other animals, and that more or less deep convexity forwards in the region of the loins has been considered by some not only as a marked character of the human spine but even as peculiar to humanity; other anatomists have denied that this is so, and consider that man and certain of the man-like apes have it in common. In this memoir Prof. Cunningham seems to minimise the importance of the lumbar curve as a distinctive character of any special group. Not only the higher, but also the majority of the lower apes, possess this curve; and, under certain conditions, even some quadrupeds show clear traces of it. In the course of his investigations, Prof. Cunningham has brought many new and interesting facts and phenomena to light. Thus in man and the chimpanzee the quality of this lumbar curve is identical; the only differences are its extent and its development. And then among the members of the human race this curve does not appear to be equally prominent; upon some —as the Australian, the Negro, and the Andaman Islander —the curve is by no means so well marked as it is in the European. Not that the absolute degree of curvature is less in these races, but whereas in the European the bodies of the vertebræ are more or less moulded in adaptation to the curve, in the lower races there is to be found no trace of this. With this subject the first part of this memoir is taken up, and the adaptation of the vertebral bodies with reference to the lumbar curve is considered in a first section. The method of making the measurements, and the results derived from them, are given, and special points in connection with the European and several of the lower races (Australian, Tasmanian, Andaman, Negro, and Bushman) are given. Then follow details of the indices of the lumbar vertebræ in the four man-like apes, as well as in nine of the lower apes. The statement that this curve is more marked in the female than in the male is strongly supported by the evidence adduced in this memoir, and it would seem that the vertebral bodies of the female are moulded more in adaptation to the curve than those of the male.

The second section of this part of the memoir treats of the entire lumbar curve as found in man and the apes. The difficulties in the way of securing accurate curvatures of the living spine seem to be insuperable. Parow, who worked hard on this subject, has signally failed; hence the standard of comparison must be sought for in the dead, and the details of how this has been done are given at some length. Racial differences are next discussed, and the development of the spinal curve is treated at great length, with some excellent illustrations. The condition of the lumbar column in the anthropoid apes is next considered. It was, as we have seen, thought that the lumbar curvature did not exist save in man. Goodsir is positive about it. Sir W. Turner at one time was equally so. Sir Richard Owen denies its presence in the gorilla and orang-utan. Huxley was among the first to assert its existence. Broca and Topinard followed. As to the facts to be seen by frozen sections, Cunningham has not succeeded in getting fresh material for the gorilla; but in the case of the chimpanzee the curve differs but little from that in man. In the orang it is feeble, resembling that in man in some respects, and in others differing from that in the chimpanzee. In a gibbon (*Hylobates agilis*) it stands intermediate between the chimpanzee and orang. In some of the monkeys it is also to be found, and even in some quadrupeds.

In a second part of his memoir, Prof. Cunningham, taking advantage of the same anatomical method which enabled him to make such interesting discoveries as to the extent of the curves of the vertebral column, viz. by sections through recently frozen bodies, has been able to advance our knowledge of the topographical anatomy of the orang, chimpanzee, and gibbon, very considerably. Certain relations of distinct morphological importance cannot by any other method be with accuracy ascertained. The question of how far the cerebrum in the anthropoid apes projects backwards in relation to the upper surface of the cerebellum, was at one time a burning question, and, although fairly set at rest, cannot be said to have been unmistakably demonstrated until now; when the whole of the parts were frozen in their places, sections were made, and we have the results in this memoir amply corroborating previous inductions. Sections of the brain *in situ* in the adult male and newly-born child, in the male and female chimpanzee, female orang, and gibbon, are all figured. Other points in the anatomy of the brain, as the condition of the corpus callosum, and of the hippocampus minor are also alluded to, and a few further details as to other visceral anatomy are given.

The memoir forms a quarto volume of some 150 pages, the typography of which is extremely creditable. The woodcut illustrations and plates are excellent, and the publication of this treatise as a Cunningham Memoir marks the appreciation of its value by the Council of the Royal Irish Academy, as the series of its publication—known as the Cunningham Memoirs, because the expenses thereof are defrayed out of the funds left by a Mr. Cunningham—is reserved only for works which the Council believe contribute some new facts to science.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, August.—Note on the eruptive rocks of the islands of Marion, Prince Edward, Macdonald, and Heard, by A. F. Renard. These insular groups, which stand on the great submarine plateau in the southern regions of the Indian Ocean, are shown to be entirely volcanic, in no way connected either with the Madagascar group or with the lands of the South Polar seas. Marion and Prince Edward, which were visited and partly explored by Mr. Buchanan, of the *Challenger* Expedition, consist of old plutonic formations, such as feldspar basalts and much more recent black and other lavas. Heard, discovered in 1853 by the American captain Heard and also visited by the *Challenger*, is largely covered with a black volcanic sand formed of grains of magnetite and augite. Elsewhere occur more recent lava formations, which show no trace of the erosive action exercised by the sands on the older rocks. All the specimens collected here belong mainly to the group of feldspar basalts.—On the presence in Belgium of *Bothriocephalus latus*, Bremser, by Edouard van Beneden. A few recent instances are recorded of the presence in Belgium of this human parasite, which is common enough in Holland.—Experimental researches on the influence of magnetism on the phenomenon of polarisation in dielectrics, by Edmond van Aubel. In this second communication the author gives the result of fresh experiments, showing how, by means of a specially-constructed electro-magnet, the electric field which interfered with previous researches may be completely eliminated, while preserving an intense magnetic field. The electro-magnet here described may also be used in ordinary physical experiments, wherever it is necessary to ascertain whether the phenomena observed with the Ruhmkorff and other electro-magnets are due to magnetism and not to the electric field or to the heat of the current traversing the bobbins.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, November 2.—M. Jurien de la Gravière, President, in the chair.—Fresh communication on rabies, by M. Louis Pasteur. (For summary of this report, see p. 30).—Note on the unequal flow of gases, by M. Haton de la Goupillière. Having, in previous papers, given a complete analytical solution of the various problems connected with this subject, the author here determines the true character of his formulas in their relation to experimental applications.—Remarks on M. Fontaine's report concerning his experiments on the transport of power by electricity, by M. Marcel Deprez. It is pointed out that M. Fontaine's method, which replaces the generator and receiver by a series of machines coupled together in sufficient number to produce the desired effect, so far from being based on any new principle, is the same as that proposed by all electricians who have sought to obtain high tensions without having recourse to the construction of the special machines first suggested by the author. The means employed by M. Fontaine to control simultaneously the four series of generators is also stated greatly to resemble that described in a patent taken out on April 28, 1885, by M. Deprez.—Experimental researches connected with the cerebral functions, by M. Brown-Séquard. These researches have been undertaken in order to show how varied and numerous are the purely dynamic effects proceeding from influences exercised on the encephalon by the sensitive nerves, and on the motor nerves by the nervous centres. Experiments carried on for seven or eight years lead to the general conclusion that all the motor nerves, and nearly all the excitable parts of the nervous centres, may have their excitability greatly modified, even under the influence of remote and slight irritations of the greater part of the nervous system.—On the atomic weight of the oxide of gadolinium, by M. A. E. Nordenskjöld. This compound is not a simple oxide, but consists of the three closely-related oxides of yttrium, erbium, and ytterbium, all with very different atomic weights. Nevertheless, even when derived from quite different minerals occurring in localities far removed from each other, it is here shown to possess a constant atomic weight. On the other hand, this substance is not a true chemical combination, but an isomorphous mixture, thus presenting a new phenomenon in chemistry and mineralogy. It is the only known instance of three isomorphous substances of the class which must still be regarded as elements that are found in Nature not only always together, but always together in like proportions. — On a new function of the otocysts in the invertebrates, by M. Yves Delage. A long series of researches recently conducted at the laboratory of Roscoff leads to the conclusions that these organs, occurring chiefly in the higher crustaceans and mollusks, serve not only as organs of hearing, but also, and perhaps mainly, as organs of locomotion, thus corresponding to the labyrinth of higher animals.—On *Gymnodinium polyphemus*, P., by M. Pouchet. Although hitherto regarded as a member of the vegetable kingdom, this organism presents the remarkable peculiarity of possessing an organ of vision of a somewhat complicated type. It is a headless Peridinian, occurring on the French coast, and, like all Peridinians, feeds on vegetables by endosmotic absorption. The eye, which always occupies a uniform position, is formed of two parts—a true crystalline and a true choroid—and its real character cannot be mistaken, resembling, as it does, in the most striking manner, the eyes of certain worms and Turbellariæ.—Saturation of selenious acid by the bases, and acidimetric analysis of this acid, by M. Ch. Blarez.—On the heat of neutralisation of the homologous or isomerous monobasic acids, by MM. H. Gal and E. Werner. The heat of neutralisation, already determined by Berthelot and Louguinine for formic, butyric, and some other fatty acids, is here determined, together with the heat of dissolution for others, such as isobutyric, isopropylacetic, trimethylacetic, caproic, &c.—Synthesis of pentamethylenediamine, of tetramethylenediamine, of piperidine, and of pyrrolidine, by M. A. Ladenburg.—On two new chlorurетted derivatives of methylbenzoyl, by M. Henri Gautier. The process is explained by which the author has obtained a trichloruretted and a bichloruretted methylbenzoyl.—A new reaction of the chloride of aluminium: syntheses of the fatty series, by M. Alph. Combes. The chloride of aluminium, which has effected so many syntheses in the aromatic series, is here for the first time systematically applied to the production of substances of the fatty series.—Hæmatoscopy, a new method of analysing blood, based on the employment of the spectroscope, by M. Hénocque. This method, already tested on 200 subjects, comprises two classes of observations: (1) determination of the quantity of oxyhæmoglobin, or active colouring-matter of the blood, by means of instruments here figured, and named "hæmatoscopes" and "hæmatospectroscopes"; (2) duration of the reduction of the oxyhæmoglobin estimated by spectroscopic examination.—Fresh remarks on the stem of Poroxylon, a fossil Gymnosperm of the Carboniferous epoch, by MM. C. Eg. Bertrand and B. Renault. By comparing together homologous sections of stems of the same order but of different periods, the authors have succeeded in determining the variations introduced by time into the normal stem of this plant.—On a fundamental condition of equilibrium for the living cells of plants, by M. Léo Errera.—Petrographic study of a carboniferous diabase from the neighbourhood of Dumbarton, by M. A. Lacroix. The rock here under consideration, a vertical greenish stratum traversing the old red sandstone, presents an opportunity of studying in a small space the various structural forms which a volcanic rock may assume under the influence of a progressive cooling process.—The dislocations of the globe during recent periods, their lines of fracture, and the conformation of the continents, by M. Jourdy.—On the unity of forces in geology (continued), by M. H. Hermite. It is argued that simple oscillations of sea-level, produced by meteorological causes, would suffice, without having recourse to internal agencies, to explain the *apparent* oscillations of the land in relation to latitude, which are characteristic of the Quaternary epoch.—On the pathologic physiology of the supra-renal capsules, by M. Guido Tizzoni.—On the contractions determined by the currents of polarisation of the living tissues, by MM. Onimus and Larat. The experiments here described place beyond doubt the existence and energy of the currents of polarisation in our tissues, thus exposing the errors of the fundamental experiments carried out by Du Bois-Raymond and most German physiologists.—Note on a remarkable substance collected at Luchon on July 28, 1885, after the fall of a thunderbolt, by M. Stanislas Meunier.

STOCKHOLM

Academy of Sciences, October 14.—Contributions to the anatomy and histology of the limnivore Annelids, by Dr. A. Wirén.—On the electric nature of drift-snow, by Prof. A. Holmgren.—On the work and activity of the Ornithological Committee of the Academy for studying birds of passage, &c., by Prof. F. A. Smitt.—On new acquisitions to the Botanical Garden of the Bergian donation, by Prof. V. Wittrock.—On the lichens of the islands of the west coast of Sweden, by Dr. P. Hellborn.—Contributions to the anatomy of the Marcgraviaceæ, by Hr. H. O. Juel.—Studies of the influence of woods and forests on the climate of Sweden, by Dr. Hamberg.—On remains of Dryasoctopetala, L., in calcareous tuff near Vadstena, by Prof. A. G. Nathorst.—On combinations of phenyl-methyl-triazol, by Hr. J. A. Bladin.—On the orbit of the comet 1877, VII., by Dr. R. Larsén.—Demonstration of the proposition that the complete integral of differential equations of the *n*th order contains *n* arbitrary constants, by Dr. G. Eneström.

BOOKS AND PAMPHLETS RECEIVED

Chemical Arithmetic, 2nd edition: S. Lupton (Macmillan).—Charter, By-Laws, and List of Members of the Institution of Civil Engineers (25. Great George Street).—Minutes of Proceedings of the Institution of Civil Engineers, vols. lxxxiii. to lxxxvi (25. Great George Street).—Lectures and Essays, 2nd edition: W. K. Clifford (Macmillan).—The Rotifera or Wheel Animalcules. part 6: C. T. Hudson and P. H. Gosse (Longmans)—Calendar of University College of North Wales, 1886-87 (Cornish, Manchester).—Persia as it is: Dr. C. J. Wills (S. Low).—High Life and Towers of Silence: Mrs. Fred Burnaby (S. Low).—Smithsonian Report, 1884, part 2 (Washington).—Proceedings of the U.S. National Museum, vol. viii., 1885 (Washington).—Practical Dynamo-Building: F. W. Walker (Iliffe and Son).—Journal of the Royal Agricultural Society, October (J. Murray).—Rendiconto dell' Accad. delle Scienze Fisiche e Matematiche, anno xxii., xxiii., xxiv., 1883-84-85; anno xxv., fasc. 1, 2, 3 (Napoli.)—Nature Musings on Holy-days and Holidays: Rev. N. Curnock (Woolmer).—Commercial Organic Analysis, vol. ii.: A. H. Allen (Churchill).—A Synopsis of Elementary Results in Pure Mathematics: G. S. Carr (Hodgson).—Our Temperaments: A. Stewart (Lockwood).—The Coming Deluge of Russian Petroleum: C. Marvin (Anderson).—Methods of Analysis of Commercial Fertilisers (Washington).—Publications of the Leander McCormick Observatory of the University of Virginia, vol. i., part 3, Nebula of Orion, 1885.

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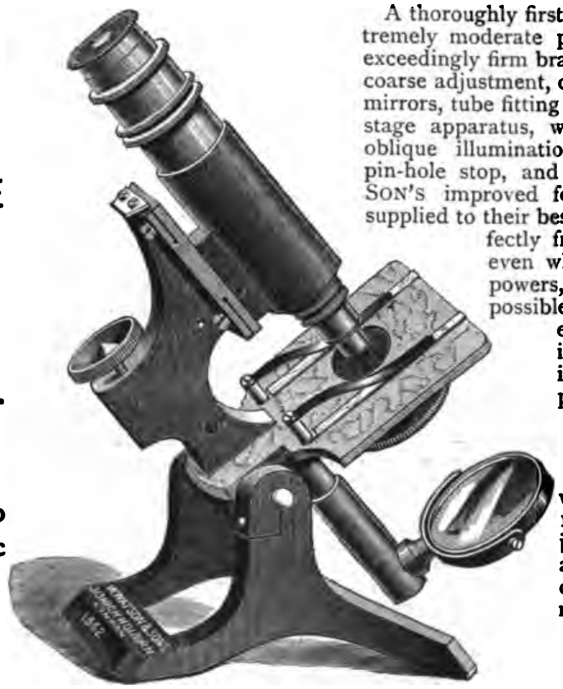

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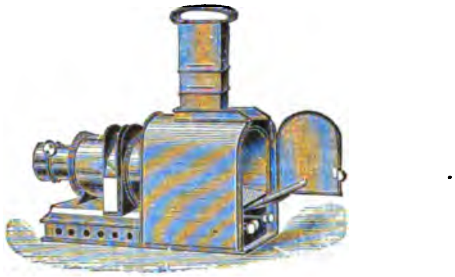

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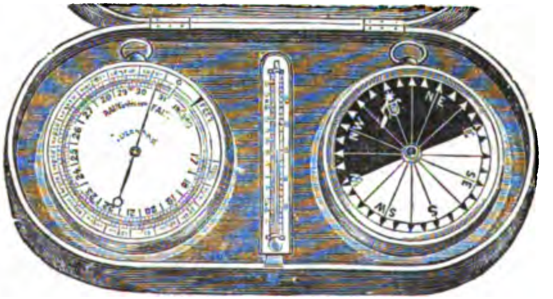

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THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873-76 *under the Command of Capt. G. S. Nares, R.N., F.R.S., and Capt. F. T. Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vols. XV. and XVI. (Published by Order of Her Majesty's Government, 1886.)

VOLUME XV. contains three Reports on the univalve Mollusca collected. The first is a short Report, by Dr. Rudolph Berg, on the Marseniadæ. This family has slowly gathered round the *Helix perspicua* of Linnæus and the *Bulla latens* of O. F. Müller. These species are found in all seas. The shell is either altogether enveloped in the mantle or is very partially exposed, always either calcareous or horny. Six genera are recognised, with 33 species, 11 of which are described and figured as new.

The great bulk of the volume is taken up with the Report, by the Rev. R. Boog Watson, on the Scaphopoda and Gasteropoda. This laborious memoir occupies over 750 pages, and is illustrated by an atlas of fifty plates. Some 1300 species, new and old, were recognised among the mass collected, and there were some 400 indistinguishable forms. In a short appendix, the Marquis de Folin describes and figures the Cæcidæ. The classification adopted, "for want of a better," is that of the Messrs. Adams. The more important of the general conclusions based on the examination of the facts attending the habitat of all the distinguished forms are as follows:—(1) Depth is an important condition in connection with Molluscan life; (2) but temperature is even a more important condition than depth; (3) great differences in either depth or temperature prove barriers to distribution; (4) where these do not exist, there would seem to be no limit to universality of distribution; (5) there are without doubt such universally distributed forms. The author sees no evidence in the oldest or most widely distributed species of any essential, lasting, and progressive change.

The last Report is on the Polyplacophora, by Prof. A. C. Haddon. The number of Chitons collected was, considering the frequency and wide distribution of the group, surprisingly small. Almost the only shore-collecting done during the cruise was on or near coral-reefs, and Chitons would seem to be rare in such places. The really deep-sea forms belong to Leptochiton, of which 4 species were found, and 2 are new. These species were taken at depths of from 60 to 2300 fathoms. Three plates accompany this Report: two are from drawings by the author, and the third, a coloured plate, gives the portraits of the new species by T. H. Thomas.

Volume XVI. contains the four following Reports:—(1) On the Cephalopoda, by W. Evans Hoyle, M.A. Oxon., Naturalist on the Editorial Staff. In a preface to this valuable Report, the author acknowledges the kindly and generous assistance which he received in its execution from Prof. Steenstrup, of Copenhagen, whose knowledge of the Cephalopods is immense, and the collection under his charge is unrivalled. This Report is almost exclusively systematic in its scope, but we are promised a supplement with anatomical details. It commences with a provisional synopsis of recent Cephalopods, which will certainly be of immense value to all workers. While it is true that no systematic treatment of the class can for some time to come be other than provisional, yet the author seems to have taken the greatest pains that his shall be as natural as possible, and until we have a nearly complete knowledge of the life-history of all the forms, more will not be attainable. This list contains 388 species referred to 68 genera. It would have been, we think, an improvement if all those collected during the Expedition had been in this list distinguished by some special mark, as well as having the recorded habitats for each given. Of the 72 species found, some 32 are described as new, and for these 4 new genera and 1 family had to be established. Out of the 388 species, some 60 or 70 had been so badly described as not to be recognisable, but of some of these no doubt the types still remain. Of the new species, none pertain to those great monster cuttles—source of many a battle. The exceedingly interesting genus Cirroteuthis is enriched with three species—one, *C. magna*, being the giant of the group, and measuring 1155 millimetres in length. The type species of this genus (*C. mülleri*, Esch.) was the only one known until 1883, when a second species was described by Fischer, and now (1885) five new species have been described by Hoyle and Verrill. The balance of the evidence seems to be in favour of all the species being deep-sea forms, though at present there are great difficulties in the way of settling the question. A new genus, Amphitretus, is established for a form in which the mantle is fused with the siphon in the median line, so that there are two openings into the branchial sac. This is a quite unique feature among Cephalopods. The species *A. pelagicus* was taken near the Kermadic Islands. It seems strange that but one specimen of *Argonauta argo* was found, for this and other species are not very rare. Of the genus Octopus, 20 species are enumerated, of which 11 are described as new. The one specimen of *Spirula peronii*, found living off Banda, is referred to, but Prof. Huxley is preparing a report on its anatomy. To the already large genus, Sepia, large additions were made, and it is interesting to note that all of the 10 new species were found between Port Jackson (Australia) and Japan. The shell, or sepiostaire, was found to present differential characters in the species, and a series of new terms has been adopted to describe its various parts. The suckers also seem to offer characters of specific importance, and so possibly will the hectocotylised arms when sufficiently known. Two species of Steenstrup's genus Taonius are recorded, one, *T. hyperboreus*, the common North Atlantic form, and the second the *T.* (*Procalistes*) *suhmi* of Lankester. Willemoes-Suhm had taken it for a Clionid Pteropod. Lankester described it as a new genus; but the author regards it as but a species of Taonius, and with this Prof. Steenstrup agrees, though he thinks that the specimens found may appertain to two species. As in the case of the Argonaut so in that of the Paper Nautilus but a single specimen was found, and that off Mataku, Fiji Islands. A most important means of obtaining specimens seems to have been by the examination of the stomachs of birds, fish, and Cetacea. These creatures seem to be much more satisfactory collectors than the tow-net, in which, though it was so constantly in use, few Cephalopods were taken. Possibly the immense activity of the cuttlefish will account for this. An atlas of thirty-three plates accompanies this Report.

(2) On the Stomatopoda, by W. K. Brooks, Johns Hopkins University. These Crustacea are in their adult forms inhabitants of shallow water. The collection brought home was but small, and contained no startling novelties, so that at first the author was somewhat disappointed; but this feeling turned to delight when he discovered that the material furnished some excellent opportunities for tracing out, and that with great completeness, the phylogeny and ontogeny of this little order. This order includes about 60 adult species and a vast number of tropical larvæ. The *Challenger* collection of adult forms consisted of only 15 species, 8 of these new, while 2 others had been, to this, very inadequately described; but the collection of pelagic larval Stomatopods was peculiarly rich, and in Mr. Brooks's hands it has yielded the material for tracing the history of several of the larval types, and also, more remarkable still, for establishing in every genus except one the connection between the adults and their larval types. The larval history of these Stomatopods has been one of the most puzzling problems in morphology, and the very admirable researches of Claus had been the only guide. Though often in error, Mr. Brooks confesses that without Claus's memoir to guide him his own labours must have failed. Unlike most Malacostraca, the Stomatopods, instead of carrying their developing eggs about with them, deposit them in their deep and out-of-the-way burrows under the water. They are thus most difficult to procure, and so difficult to rear that probably not a single instance of a young Stomatopod being reared from the egg is known. The growth of the larvæ is slow, and the larval life long, and, as they are as independent and as much exposed to changes in their environment and to the struggle for existence as the adults, it has come to pass that they as larvæ have undergone countless modifications which have no reference to the life of the adult, and are therefore unrepresented in the adult organism, in which the larvæ differ *inter se* more than the adults do, thus reversing the general rule. The history of each larval type has thus to be traced by the selection and comparison of those larvæ which belong to the series, and in doing this the author was partly guided by general resemblances and partly by a series of comparative measurements. The differences between the genera are slight, and all can be grouped into a single family, Squillidæ. In the description of *Lysiosquilla excavatrix* we have a very interesting account of its habits. The Report concludes with an elaborate account of the various larval forms and their adult connections. Sixteen plates accompany the Report.

(3) On the Reef Corals, by John J. Quelch, B.Sc. Lond. The author apologises for this Report of just 200 pages being so short, as he was limited both as to time and space. A careful perusal of the memoir inclines us to the opinion that no such apology is needed. Without being a monograph, the Report forms a most important contribution to our knowledge, and this not only of the distribution of the reef corals, but also, in many instances, of their structure. The term reef coral is undoubtedly vague, but the forms described in this report belong almost entirely to the Reef Madrepores, descriptions of some few species of Millepores being added. The collection made contained representatives of 293 species referable to 69 genera, and many of the species were represented by series of specimens often presenting a considerable degree of variation. As a proof of how little known are the corals of the Pacific and Indo-Pacific Islands, it may be mentioned that 71 of the new species were found in these regions, while but 2 were from the Atlantic. No attempt has been made to describe the soft parts of the specimens. Special attention is directed to the fact that the descriptions of species apply to specimens in which the calycles are perfect; in most museum specimens these are generally to be found greatly injured, and then it is often impossible to distinguish between closely-related forms. In the treatment of the distribution of these corals, lists are given of the species obtained at each locality, together with lists of the new species, and of old species recorded from the stations for the first time. While the classification adopted is on the main based on that of Milne-Edwards and Haime, a rather startling novelty in arrangement is the merging of the Madreporia Rugosa with the section of Madreporia Aporosa. The detailed reasons for this are given on pp. 40-43; and as a result the author considers that there is not a single characteristic of the old group Rugosa which will essentially separate forms usually included under it from the more typical Astræids. Thus in many Astræids the septa present are not multiples of six, while in some typical Cyathophyllidæ the septa are simply radially arranged, without any indication whatever of a tetrameral type. Again, the presence of a fossula is scarcely even of generic value; and as to the presence in the adult rugose coral of but two sizes of septa, this phenomenon is not always present in the species, and is to be met with in some typical Astræids; while as to the tabulæ, which are no doubt very characteristic of the Rugosa as a group, still even these are present in some Astræids, and absent in some Rugosa. In a striking new Madrepore, *Moseleya latistellata*, the characters are to a marked degree intermediate.

There is yet a great deal of work to be done ere the distributional areas of the reef corals is known. Probably the coral fauna of no district, unless that of the Red Sea, has been fairly worked out. It was in the nature of things that the cruise of the *Challenger* could not, from the shortness of its sojourn at any one coral district, do much in this direction. Still some few facts of great interest have been brought to light, one of the most remarkable being the occurrence of an undoubted reef-building species, *Manicina areolata*, in Simon's Bay, between lat. 34° and 35° S., at a depth of from 10 to 20 fathoms, and at a temperature of 65° F., and this is all the more peculiar, as this coral is a well-known West Indian reef-building form. Another coral, *Cladocera arbuscula*, was also found at Simon's Bay, though a West Indian species.

Notes and descriptions of eight species of Millepora are given in an appendix. One new species is called after Mr. Murray, being the one on which he saw the living zooids of this remarkable group of Hydroids. It was *M. nodosa*, occurring at Tahiti, that afforded Prof. Moseley the material for his brilliant confirmation of the observations of L. Agassiz. Twelve plates, the figures on which are beautifully executed by Mr. H. Gawan, accompany this Report.

The concluding Report in this volume is by Prof. Sir William Turner, being on the Human Crania, &c., collected during the cruise. This forms Part 2, being on the bones of the skeleton, and is an Essay on the Comparative Osteology of those Races of Men whose bones are described in the Report, for it incorporates the study not only of the material collected during the cruise of the *Challenger*, but that brought together by the authors' eminent predecessors in the Chair of Anatomy in the University of Edinburgh.

Just a century ago Camper pointed out some of the differences existing between the pelvis of a Negro and a European, and since then a vast amount of information on the subject has been accumulated, and so far as the races described in this Report are concerned, it has been exhaustively treated by Sir W. Turner. He classifies the pelvis into three groups: dolichopellic, with a brim index above 95; mesatipellic, with a brim index from 90 to 95; and platypellic, with index below 90. As to the race and age characters of the pelvis, the details, however interesting, are too technical to be abstracted. In reference to the question of how far the mode of life may act as modifying the transverse diameter of the pelvic brim, we may add that the expression "to sit on one's hunkers," would be readily understood in the North of Ireland, where it is an attitude strictly forbidden to young people. In the section treating of the spinal column, the subjects of peculiarities of individual vertebræ and the lumbar curve are investigated; and in another section the scapula, inferior and superior extremities, are examined. In a concluding section we have a general summary, and an appendix to the first part of the memoir on the "Human Crania," in which some additional details are given of some crania from Australia, the Sandwich Islands, New Guinea, and Fuegia. An index to both parts also accompanies this Report, which is illustrated by three plates of the pelves of different races.

The greater portion of the manuscript of these two large volumes was handed to the editor between July 1885 and July 1886, and the editor is to be congratulated on the successful manner in which this immense amount of scientific matter has been seen through the press.

ELEMENTARY DYNAMICS

Lessons in Elementary Dynamics. Arranged by H. G. Madan, M.A., Assistant Master in Eton College. Pp. 180. (Edinburgh: W. and R. Chambers, 1886.)

IN this little book the author has provided teachers of elementary mechanics with a rich storehouse of materials for experimental demonstrations, although the work is not quite satisfactory in some other respects. His endeavour has been to explain some of the properties of matter, Newton's laws of motion, and the modern conceptions of energy and work, in such a manner as involves only the most elementary knowledge of mathematics. Thus symbolical reasoning and formulæ are dispensed with, and nothing assumed beyond a knowledge of arithmetic and a little easy geometry. There is a successful attempt made to arouse a real interest in the subject by continual reference to phenomena of every-day life, and especially by illustrations drawn from the sports and games of the pupils. In some cases detailed instructions are given for performing the experiments. These are valuable, and similar aid might with advantage be provided in many other instances.

The author is of opinion that mechanics ought to have the first place in a boy's scientific education. This position would be strengthened, if some series of simple experiments, to be performed by the pupils themselves, were provided, and regarded as essential.

Some expressions, such as "above," "below," "on the same level," which are usually left undefined, have their exact scientific meaning pointed out. On the other hand, there is occasionally looseness and confusion in the use of technical terms. For example, in Section 103 we read: "Momentum is the term used to express the *force* with which anything is moving." In Section 159 we have the accurate statement that, by finding the momentum of a body, we learn what *impulse* has been applied to it: here the accepted expression for the time-integral of a force is used, but we do not notice any definition of the word "impulse"; and the exposition of the second law of motion appears vague in consequence. Similarly, the *force* exerted in throwing a cricket-ball is spoken of in Section 156, where the time-integral of the force is in question.

Section 302 is devoted to the "exact valuation of the energy in a moving body," and the usual expression—energy = $\frac{1}{2}$(mass × velocity^2)—is obtained, but by a process which is at least startling. Witness these statements:—"If the work could be done *in an instant*, the energy would be exactly expressed by the product of the mass × velocity^2;" and again, "The whole amount of work which a moving body can do in the time during which its motion is being stopped will correspond to the *average* or *mean* amount of energy between that which it has at the beginning of the time and that which it has at the end of the time." *Unde, quo veni?*

After the preceding, it is a small matter to refer to Section 311, where this statement occurs: "The motion of the pendulum is an accelerated motion, and, as in all other uniformly accelerated motions, the spaces described are as the squares of the times." Here, of course the reasoning is fallacious; and, although the proof intended is sound, it involves the doctrine of limits, and wants development. It is surely better at this stage of the pupil's progress to rely on the experiments in Section 312.

There is an appendix on the metric system, and, in conclusion, a dozen pages of questions and exercises on the several chapters of the book. A. R. W.

OUR BOOK SHELF

Food-Grains of India. By A. H. Church, M.A. (London: Chapman and Hall, 1886.)

A WELL-WRITTEN, well-illustrated, and well-turned-out volume. Its thinness only enhances its elegance. Its illustrations, by Mr. G. W. Ruffles, are charming, clear, without hardness, and life-like. The text is interesting, and the number of food-grains described in excess of what most of us were aware existed. Prof. Church commences his work by what must have been to him a familiar task—describing the chief constituents of food, splitting up the sugars into their groups, and pointing out the differences between true nutrients and food-adjuncts. Part 2 is devoted to dietaries and rations. With Part 3 commences the peculiar merit and *raison d'être* of the work. After some remarks upon cereals generally, the reader is introduced *seriatim* to no fewer than twenty-three cereals, the only member of the group conspicuous by its absence being rye—a grain which occupies a very important place in Europe. The presumption is that it does not occur in India, but such a presumption surely presumes too much. Wheat is described as an annual grass of unknown origin, but we scarcely see why this *nescience* as to the origin of wheat should be especially set forth. Are we to infer that barley, oats, maize, rice, the millets, &c., are annual grasses of known origin? If so, would that the Professor had devoted a few lines in each case to this particular point! The origin of our food-grains is a deeply interesting subject, veiled, we are afraid, for the most part in mist, and only conjecturally outlined.

The author disclaims any special originality, and duly credits the works of Dr. Forbes Watson, and Messrs. Duthie and Fuller, as well as other authors, as sources from which he has industriously gathered information. Messrs. Duthie and Fuller's work, however, dealt but little with the chemistry or physiology of the plants they described, and they treated more exclusively of the cultivation of the various crops.

The interest of Prof. Church's book lies in the illustrations, which are super-excellent; in the analyses, many of which were made in the author's own laboratory; in fixing the nutrient-ratio and nutrient-value of so many foods; and, lastly, in the comprehensive view given of Indian cereal and other crops. The Indian local names and Sanskrit equivalents are also interesting. These are taken by our author on trust, but all or many of them also occur in Mr. Duthie's book, which would be a guarantee of their correctness.

JOHN WRIGHTSON

Tobacco a Farmer's Crop. By Philip Meadows Taylor. (London: Edward Stanford, 1886.)

THIS is a small book of seventy pages. The first half is occupied by pleasant matter relating to the history of tobacco in Europe not strictly or seriously relevant to the title. The latter half redeems the whole from the stigma of being unpractical. An interesting account is given of the despotic regulations of the "Régie des Tabacs," a Government Department which grants licenses for growing, manufacturing, and selling tobacco throughout France, and whose powers extend to the nomination of the cultivators, the variety of tobacco to be grown, the number of plants per hectare, and even the number of leaves permitted per plant, so that the unfortunate cultivator may and must give a perfectly accurate account of his yield down to a single leaf. The methods of cultivation followed in France are described plainly and apparently practically. The important question as to whether tobacco can be grown profitably in England is answered unhesitatingly in the affirmative, and a sensible scheme is propounded for bringing its culture into harmony with the Excise. The coldness exhibited by our Royal Agricultural Society towards the tobacco movement last April is strongly animadverted upon. As to our climate, Mr. Taylor writes as follows:—"It is stated to be too cold, too damp, too uncertain in England to allow of the introduction of the proposed culture. I cannot conceive or allow that there can exist any sensible difference between the climate of the southern counties of England and that of Picardy and Flanders. I do not take notice of Prussia and even Russia, where tobacco is grown. I believe that the general climate in Southern England is more genial than in the countries across the Channel, and I feel confident that in the said southern counties of England and in Ireland tobacco could be advantageously grown. I recall my former statement that the plant is only on the ground from June to September: cold winters, early frosts, and November fogs have naught to do with the question." The author does not appear to take into account the comparative coolness of the summer months in England, which has always prevented the successful growth of maize, vines, and probably tobacco also. This very readable little book, with its unstudied side-lights upon French rural life, and its pleasant style, may be recommended without any hesitation to the reading public.

JOHN WRIGHTSON

Marion's Practical Guide to Photography. (London: Marion and Co., 1886.)

HERE we have a very good book, which contains all necessary information and useful hints for those who are practising the art of photography. The whole process is gone through in a very clear and easy way. Extra chapters are given on different parts of the subject, such as photographic optics, re-touching, portraiture, &c. On p. 95 a table of exposures is added, preceded by explanations, taking into consideration all the variations of scenes and subjects which the amateur is likely to come across. The manufacturers deserve great credit for publishing a book in which the best way of using their apparatus is described; a book published under such conditions ought to be truly practical, and one would think that the manufacturer of bad apparatus would not be too anxious to teach his customers how to find it out.

Lecture Notes and Problems on Sound, Light, and Heat. By Charles Bird, B.A., F.G.S. (London: Relfe Bros., 1886.)

FOR students who are attending lectures on these subjects this book will be very useful, as it contains the chief fundamental formulæ, set out in a very clear manner, and it is very compact, capable of being put into one's pocket without inconvenience.

Bicycles and Tricycles for the Year 1886. By H. H. Griffin, London Athletic Club. (London: L. Upcott Gill, 1886.)

NOW that cycling has become so general, and consequently the cycle industry increased so largely, a book on the subject will doubtless be most welcome. We have here one which gives a *good* description, and in many cases a woodcut, of every known make, with the exception, perhaps, of one or two very new patterns which have been introduced very recently. We need not enter into the details from the scientific point of view, as they have been previously described (NATURE, vol. xxxiii. p. 132). A description of different varieties of bells, lamps, &c., is also given. Great pains seem to have been taken by the author to bring the book up to date, and to give an accurate account; each machine, as he tells us, having been examined by himself.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Extension of the Corona

IN reference to the failure of observers at the late eclipse to note any such extension of the corona as was seen in 1878, I desire to add my testimony to Mr. Common's opinion (NATURE, vol. xxxiv. p. 470), that the conditions of the sky must have been wholly different; and where the visibility of the corona is in question, the atmospheric diffusion is all-important.

We have a most trustworthy criterion of the amount of diffused sky light in the visibility of the moon's limb outside the sun on the coronal background. This appears not to have been observed at all last August, and it may be useful to recall what it looked like under certain almost ideal conditions, which are not likely to recur.

On July 29, 1878, I observed it in the remarkably clear air of Colorado, and at an altitude of over 14,000 feet, on Pike's Peak, and have a vivid recollection of its appearance then. After totality, and while writing my notes, I heard a call from some bystander of "Look at the moon!" and glancing up from the paper (with an eye which could not have been in a sensitive condition), saw the moon's limb outside the sun, most conspicuously defined by a band of pearly light, which faded outward, but whose visible width can be estimated from the fact that though I went on intermittently with my notes, and took no other precaution to shield the eye than keeping it in the shadow cast by my telescope stand, the limb continued in my view under these unfavourable circumstances for *four minutes and twelve seconds* after totality was over. A similar duration was recorded by Gen. Myer, the Chief Signal Officer of the United States, who observed near me; and others at a lower altitude certified to having observed it over three minutes. Something is due to the increased sensitiveness of the eye after the darkness, but there is no doubt that, with even the slight rest of the retina which totality afforded, the phenomenon was such a salient one as to force itself on the attention of those not regarding it.

This is for a very exceptionally pure sky, of course; but if, as is stated, observers specially seeking it could not even see the limb a little outside the sun (where the corona is brightest) last August, it seems clear that no conclusions as to its non-visibility under any ordinary means are to be drawn from negative evidence of such a kind. S. P. LANGLEY

Allegheny Observatory, Allegheny, Pennyslvania

The Astronomical Theory of the Great Ice Age

IN your issue of November 4 (p. 7), my friend Mr. W. H. S. Monck asks one or two questions relative to the paper on "The Astronomical Theory of the Great Ice Age" which you did me the honour to reprint.

I take as a convenient unit the mean daily sun heat on one hemisphere. The amount of this unit is indicated by the fact that it continuously maintains the earth's temperature some 300° more or less above what it would be were the sun's heat withdrawn.

The calculations I gave showed that in the glacial winter the mean daily receipt of heat sunk to ·68 of a unit, while in the brief glacial summer the mean daily receipt was 1·38 unit.

Considering the magnitude of the unit, it is obvious that fluctuations like this must correspond to vast climatic changes of the kind postulated in the Ice age. Here it seems to me lies the great originating cause of the Ice age, and to dwell on the minor phenomena merely obscures the real point.

If it be said that no great climatic change takes place because the total sun heat in the year remains the same, then I remark, as I did at the Royal Institution, that on this principle it would be the same thing to give a horse 15 lbs. of oats a day for six months and 5 lbs. a day for the other six months as to give him 10 lbs. of oats a day all the year round. ROBERT S. BALL

The Observatory, co. Dublin, November 11

P.S.—I take this opportunity of correcting a misprint in my paper as given in NATURE (vol. xxxiv. p. 608). The maximum number of days' difference between summer and winter is 465 × eccentricity.

Abnormality in Cats' Paws

AMONG the many interesting features suggested by the genealogical table in last week's NATURE (p. 40), showing the persistence of abnormality in the number of toes on a cat's paw, there is one the significance of which seems not to have occurred to, or to have been passed over by, Mr. Edward Poulton. The peculiarity I refer to is the larger percentage of abnormality among the female offspring than among the male. Taking "Tabby V." as a starting-point, and leaving out one abnormal kitten of which the sex was unnoted, as well as the families of which no particulars are given, the total number of descendants in the table is 36, of which 12 are males and 24 females. Of the 12 males, 5 are normal and 7 abnormal, or 41⅔ and 58⅓ per cent. respectively; and among the 24 females 7 are normal and 17 abnormal, or 29⅙ and 70⅚ per cent. respectively. Or, to put it in another way, among the 12 normal kittens 5 are males, 7 are females, or 41⅔ and 58⅓ per cent. respectively, instead of 33⅓ and 66⅔ per cent. as it should be; and among the abnormals 7 are males and 17 females, or 29⅙ and 70⅚ per cent. instead of 33⅓ and 66⅔.

This would seem to indicate either (1) that there is a greater tendency among the male offspring than among the female to revert to the normal condition, or (2) that there is a tendency among the offspring to inherit rather the peculiarities of the parent of their own sex—the male parent in all cases in the table being assumed to be normal. If rather, probably the former, though the latter could easily be tested by a similar set of observations with cats, the male parent of which was abnormal, the mothers being in each case normal.

J. HERBERT HODD

Hatton Garden, London, E.C., November 15

Abnormalities in the Vertebral Column of the Common Frog

IN preparing skeletons of the frog, my students came across the following abnormalities in the vertebral column, a record of which may be not without interest:—

(1) In a large *Rana temporaria*, the centrum of the eighth vertebra, instead of being biconcave (amphicœlous), is concavo-convex (procœlous), like that of the preceding vertebra. This abnormality I have observed before.

(2) In a medium-sized *Rana temporaria*, the eighth and ninth vertebræ are both abnormal. The ninth vertebra has well-developed only one transverse process (the right) for articulation with the ilium. The other (the left) is quite small and ill-shapen; there is is no anterior zygapophysis on this side. The centrum is anteriorly convex on the right side and concave on the left side. Posteriorly, there is on the right side a convex articular surface for the urostyle; but on the left side the articular surface is ill-developed and irregular. In the eighth vertebra, the left transverse process is abnormally large and strong, has a marked backward direction, and has taken on itself the sacral function on this side, articulating with the ilium. The right transverse process is nearly, but not quite normal. There is a right, but no left, posterior zygapophysis. The anterior end of the centrum is normally concave; but the posterior end is convex on the left side and concave on the right side. The urostyle and the ilia are slightly modified in accordance with the abnormalities of the vertebræ. C. LLOYD MORGAN

University College, Bristol

Influence of Wind on Barometric Readings

ALLOW me a few words of supplement to Prof. Abbe's useful letter in NATURE of November 11, p. 29.

Sir H. James's paper is perhaps better known on this side of the Atlantic than Prof. Abbe thinks; but there undoubtedly is too great a tendency to rush into print without previously reading up what has been done. The great bibliographical work which the Signal Office has in hand will do more to check this evil than anything which could be suggested, and hence its enormous importance.

As regards the application of suction to anemometers, no reference is made to that of Bourdon,[1] of which my friend Dr. Fines was recently kind enough to show me a very fine specimen at work at his observatory at Perpignan.

The Cowl Committee of the Sanitary Institute, far from being (as has been imagined) asleep or dead, has been very hard at work, and will in a few months report the result.

I sincerely hope that Lord Rayleigh will accede to Prof. Abbe's suggestion, but in the interim I append the report of Lord Rayleigh's Southampton paper which appeared in the *Meteorological Magazine* for October 1882, p. 130:—

"'On the Effect of Wind on the Draught of Chimneys,' by Prof. Lord Rayleigh, F.R.S.

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[1] See also Laughton, "Historical Sketch of Anemometry," *Quart. Journ. Roy. Met. Soc.* vol. viii. (1882), p. 177.

"The author said that the draught diminished as the direction of the wind was more and more downwards, but did not go backwards until the inclination amounted to about 30°. The maximum up-draught would occur, not, as was often supposed, with a direction of wind vertically upwards, but with one making an angle of about 30° with the vertical. A chimney with a T-piece at the top never produced an unfavourable effect on the up draught, and only in one case failed to produce a favourable one. With a T-piece to which was affixed vertical ends, every wind met with would have a favourable effect, and no wind known would have an unfavourable effect.

"Prof. De Chaumont thought that vertical ends increase the resistance of the up-draught, and described a chimney with a lamp-shade-like top and conical cap, with which it was impossible to get a down-draught." G. J. SYMONS,

62, Camden Square, N.W. Registrar Sanitary Institute

Barnard's Comet

I WONDER that more has not been written about Barnard's comet (*f* 1886). On the 9th, at 17h. 50m., in spite of the strong twilight, it was plain with the naked eye as a star. I did not notice its exact brightness, but it was perhaps equal to ρ Virginis. With the telescope its head was about 8′ diameter, and it had two faint tails at about position-angles 250° and 300°. The former, which was the brighter at its origin, was ½° long, and was straight; the latter I believe was curved, and was 1½° long. T. W. BACKHOUSE

Sunderland, November 11

Aurora

LAST evening (November 2), between the hours of seven and eight o'clock, a bright aurora was visible in this vicinity. At intervals later in the evening, patches of cirrus clouds in the northern sky became luminous. The disturbance of the suspended magnet was at its height early in the evening, when the aurora was brightest. It is interesting to note the fact that this aurora was twenty-six days removed from that of October 7 and 8, corresponding to the time of the revolution of the sun on his axis. It is noteworthy, also, that very near to the time of the appearance of each aurora there was a slight renewal of earthquake activity in South Carolina and other localities.

Lyons, New York, November 3 M. A. VEEDER

"Lung Sick"

DR. EMIL HOLUB, in writing to me a few months ago from Panda-ma-Tenka, Albert Country, Zambesi, mentions having treated his cattle in a similar manner to that referred to in NATURE of the 11th inst. (p. 29). He says:—

"Shortly after I started northward from the Vaal, a contagious disease broke out among my cattle; there was any amount of sickness among the numerous trains (forty teams a day) going to the Diamond Fields, but I could get no clue to the lameness of the front legs of my bullocks for a long time. Having shot one, the disease proved to be a contagious pleuro-pneumonia, similar to the 'lung sick' so prevalent in this neighbourhood, affecting hips and shoulder-blades, causing lameness. The lungs were partly destroyed, but the animal had but little cough. I disinfected the whole herd, and vaccinated the healthy as well as the sick. The end of the tail was pierced with a narrow-bladed dagger, and a piece of lung full of virus inserted and then bandaged. The second vaccination effectually prevented the spreading of the disease for the whole journey, even in native locations similar to the Bechuanas, in which we were surrounded with 'lung-sick' cattle dying near our encampment." PHILIP J. BUTLER

55, De Beauvoir Road, London, N., November 13

PAUL BERT

PAUL BERT, who has died at his post as Governor of Tonquin, was born at Auxerre in 1833, graduated Doctor of Medicine in 1863, and Doctor of Science in 1866. Obtaining a professorship in the Faculty of Science at Bordeaux, M. Bert devoted himself especially to physiology, and in 1869 he obtained the Chair of General Physiology in the Faculty of Science at Paris. He continued here his experiments on the influence of changes of barometric pressure on life, and presented a series of papers on the subject to the Academy of Sciences, which awarded him, in 1875, its great biennial prize of 20,000 francs. He entered political life in 1870, and has all along been known as an advanced Radical. He, however, never lost his interest in science; he did much to promote education in France, and took an active part in the legislative movement which obtained for M. Pasteur an annual pension of 12,000 francs as a national recompense. M. Bert was elected President of the Biological Society in 1878, in succession to Claude Bernard, whose most brilliant pupil he was, and more recently was admitted to the Academy of Sciences. In Gambetta's Cabinet of 1881 he was Minister of Public Instruction, and a few months ago accepted the post of Governor of Tonquin, where one of his most notable acts was the founding of a Tonquinese Academy. M. Bert's papers on "Barometric Pressure" were published as a separate volume in 1877, and his lectures at the Museum of Natural History were in 1869 published under the title of "Leçons sur la Physiologie Comparée de la Respiration." He also issued, in 1869-70, "Notes d'Anatomie et de Physiologie Comparées." For many years he had charge of the scientific department of the *République Française*.

At the sitting of the Academy of Sciences on Monday, the President, M. Jurien de la Gravière, expressed regret that politics had diverted M. Paul Bert from physiology; and M. Vulpian remarked that his death, though glorious for the country, was a calamity for science, his numerous memoirs having placed him among the first physiologists of the age. The Academy adjourned in sign of mourning.

THE RECENT WEATHER

AT the close of a short period of somewhat unusual weather conditions, it may be worth while to call attention to the more prominent features of those conditions.

Cyclonic systems, some of wide, some of small dimensions, have been primarily developed over Western Europe in unusually large numbers. Opportunities for studying those atmospheric conditions from which barometric depressions originate within the area of our European stations are by no means very rare, but they are nevertheless sufficiently scarce to merit careful scrutiny at the hands of every student of weather knowledge. So much is this the case that a meteorologist of eminence made, some years since, the statement that no one had ever been present at the birth of a storm.

Considering the disastrous nature of the floods, the sloppiness of earth and sky, and the general misery in the aspect of things, which characterise the event, few of us can wish to be very frequently spectators of it. But when it occurs, the conditions accompanying it should be carefully attended to. These may perhaps be briefly summarised thus:—

(1) Barometric depressions are primarily developed over a region where atmospheric gradients are slight, the exceptions to this rule being those systems (secondary or subsidiary, as they are termed) which first appear as loops or bulges in the isobars of a large pre-existing cyclone.

(2) They originate either in the rear of a depression which has already passed away or in the inter-space between two large anticyclones, and more especially when the anticyclones are so large that this inter-space constitutes what is called a "trough" of relatively low pressure.

(3) They are preceded and accompanied by an enormous condensation of vapour into cloud.

(4) They do not, at the moment of their birth, appear

to affect the upper currents of the atmosphere, but, if growing large, soon afterwards do so, in such a manner that the hypothetical isobars at the level of the cirrus appear to be bent into a V-shaped hollow from the great polar depression to a point nearly above the centre of the circular depression at the earth's surface.

Of the depressions lately developed near the British Isles and over the south-west of France, the greater number originated in "troughs" of relatively low pressure, such as have been above alluded to: and their movements may be said to have been unusually erratic. Yet they obeyed the ordinary rule of progression, in so travelling, as to have the highest general pressures on the right of their course. Thus, those depressions which originated near the east side of a "trough" lying north-north-west to south-south-east, tended to move to north-north-west, while those which originated on the other side of the "trough" travelled in the contrary direction. On the 10th inst., Great Britain lay between two depressions travelling in parallel but opposite directions, these directions being transverse to the mean direction of movement prevalent at this season of the year. The same phenomenon was repeated on the 12th inst. It is now more than twenty years since I began constructing daily charts of the directions taken by European depressions, and during the whole of that period there has been no instance entirely comparable with these.

We must wait for reports from a very extensive portion of the earth's surface before a comprehensive study of these phenomena can be undertaken. Two questions, so correlated that they may require but a single answer, are of prime importance:—What causes the abnormal, but temporarily persistent, determination of aqueous vapour to certain portions of the globe? What causes the abnormal, but temporarily persistent, occupation of certain portions of the globe by anticyclonic systems?

W. CLEMENT LEY

THE WORK OF THE UNITED STATES FISH COMMISSION[1]

A BRIEF memorandum of what the U.S. Fish Commission hopes to accomplish in time, in connection with its mission, is as follows:—

(1) In the department of investigation and research there is yet to be carried out an exhaustive inquiry into the character, abundance, geographical distribution, and economical qualities of the inhabitants of the waters, both fresh and salt. The subject is practically unlimited in extent, and, so far as the ocean is concerned, has scarcely been touched. With the powerful apparatus, however, at the command of the Commission it is expected that much progress will be made year by year, and that the publication of the results and the distribution of duplicate specimens to colleges and academies in the United States will be carried out on a large scale, so as to meet a large and increasing demand from teachers and students.

(2) A second object, in connection with the sea fisheries, is the improvement of the old methods and apparatus of fishing and the introduction of new ones.

The work of the Commission in bringing to the notice of American fishermen the importance of gill-nets with glass-ball floats for the capture of codfish has already revolutionised the winter cod-fishery industry in New England. Looked upon almost with ridicule by the Gloucester fishermen, when first brought to their notice by the Commission, these nets have come rapidly into use, until at the present time they represent the most important element in the winter fisheries, the number of fish taken being not only much greater than heretofore but the fish themselves of finer quality.

The ability to maintain a successful fishery without the use of bait is of the utmost importance, in view of the fact that when the cod are most abundant bait is almost unprocurable. Other forms of apparatus of less importance have also been introduced, and a constant look-out is maintained, by correspondence and otherwise, in connection with the improvement of fishing machinery.

(3) Another important point for consideration is that of improvement in the pattern of fishing vessels. There is annually a terrible mortality in the fishing crews of New England, especially those belonging to the port of Gloucester, to say nothing of the total loss and wreck of the fishing vessels and their contents. There has gradually developed in connection with the mackerel and cod fisheries of New England a pattern of vessel which, while admirable for speed and beauty of lines and of rig, is less safe under certain emergencies than the more substantial and deeper vessel used abroad, especially in England and Scotland.

The subject of the best form of fishing vessel has been intrusted to Captain Collins, of the Commission, himself a most experienced fisherman, and, after a careful study of the boats of all nations, he has prepared a model which is believed to combine the excellences of both English and American vessels.

An appropriation will be asked from Congress for means to construct an experimental vessel and test its qualities; but until a successful experiment has been made it will be difficult to induce the fishermen to change their present form of construction.

(4) The fourth object of the Commission is to determine the extent and general character of the old fishing localities and to discover new ones. There is no doubt whatever that there still remain many important areas, even in the best-known seas, where the codfish and the halibut will be found in their former abundance. There has never been any formal investigation on this subject, and the banks that are known have been brought to light purely by accident. It is believed that by a systematic research and a careful survey the area of known grounds can be greatly extended.

There is very great reason to hope for successful results from this inquiry in the waters off the South Atlantic coast and in the Gulf of Mexico. These regions, the latter especially, may be considered as practically unknown, the few established localities for good fishing being in very small proportion to what must exist. It is here that the service of the fishing schooner referred to above, if means can be obtained to build it, will be brought into play, and it is not too much to hope that an industry will be developed that will represent to the Southern and South-Western States the same source of income and occupation that the mackerel, cod, and halibut furnish to the fishermen of New England.

(5) There is also much to be learned in the way of curing and packing fish for general and special markets. The American methods have grown up as a matter of routine, and are adapted to only one class of demand. There are, however, many modes of preparation which can be made use of to meet the wants of new markets; and thus we can enter more efficiently into competition with European nations for European trade, as well as for that of the West Indies and South America.

A great advance has already been made towards this desired improvement since the Centennial Exhibition of 1876, where many methods of curing and putting up fish were shown in the foreign sections that were almost entirely unknown in America. Notable among these were the preparations of sardines and other species of herring in oil, as well as in spiced juices. Quite recently this industry has been well established in Maine, amounting to a value of millions of dollars, and there are many other parts of the country where the same work can be done with other kinds of fish. The whole subject is receiving the careful consideration of the Commission, and numerous facts bearing upon it have been announced in its reports and bulletins.

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[1] From the "Report of the U.S. Commission of Fish and Fisheries" (Washington, 1885).

(6) The work of increasing the supply of valuable fishes and other aquatic forms in the waters of the United States, whether by artificial propagation or by transplantation, although very successful, may be considered as yet in its infancy.

It must be remembered that the agencies which have tended to diminish the abundance of the fish have been at work for many years and are increasing in an enormous ratio. This, taken in connection with the rapid multiplication of the population of the United States, makes the work an extremely difficult one. If the general conditions remained the same as they were fifty years ago, it would be a very simple thing to restore the former equilibrium.

At that time, it must be remembered, the methods of preservation and of wholesale transfer, by means of ice, were not known, while the means of quick transportation were very limited. Hence a small number of fish supplied fully the demand, with the exception, of course, of species that were salted down, like the cod, the mackerel, and the herring (including the shad). Now, however, the conditions are entirely changed. The whole country participates in the benefits of a large capture of fish, and there is no danger of glutting the market, since any surplus can be immediately frozen and shipped to a distance, or held until the occurrence of a renewed demand.

Another impediment to the rapid accomplishment of the desired result is the absence of concurrent protective legislation of a sufficiently stringent character to prevent unnecessary waste of the fish during the critical period of spawning, and the erection or maintenance of impediments to their movements in reaching the spawning-grounds. This is especially the case with the shad and the salmon, where the simple construction of an impassable dam, or the erection of a factory discharging its poisonous waste into the water, may in a few years entirely exterminate a successful and valuable fishery.

It is to be hoped that public opinion will be gradually led up to the necessity of action of the kind referred to, and that year by year a continued increase in the fisheries will be manifested. Even if this does not occur as rapidly as some may hope, the experiments so far furnish the strongest arguments in favour of continuing the work for a reasonable time. A diminution that has been going on for fifty or more years is not to be overcome in ten, in view of the increasing obstacles already referred to.

Among the species an increase of which in their appropriate places and seasons is to be hoped for, in addition to those now occupying the attention of fish-culturists, are the cod, the halibut, the common mackerel, the Spanish mackerel, the striped bass, or rockfish, &c.

One of the most important, and at the same time among the most promising, fish is the California trout, with which it is hoped to stock large areas of the country. Its special commendations are mentioned elsewhere in this Report.

Another fishery earnestly calling for assistance, and capable of receiving it, is that of the lobster, the decrease of which has been very marked. The experiments of the Fish Commission suggest methods by which the number can be greatly increased. Something, too, may be done with the common crab of the Atlantic coast and its transfer to the Pacific. Some kinds might also be advantageously brought to the eastern portion of the United States from the Pacific coast and from the European seas.

A subject of as much importance as any other that now occupies the attention of the Fish Commission is an increase in the supply of oysters. In no department of the American fisheries has there been so rapid and alarming a decrease, and the boasted abundance of this mollusk on the Atlantic coast, especially in Chesapeake Bay, is rapidly being changed to a condition of scarcity which threatens practical extermination, as is almost the case in England A fishing industry producing millions of dollars is menaced with extinction, and needs the most stringent measures for its protection.

The U.S. Fish Commission has been very fortunate, through its agents and assistants, in making important discoveries in connection with the propagation of the oyster, which are referred to hereafter in this Report; and it is proposed to establish several experimental stations for applying the discoveries thus made, so as to constitute a school of instruction and information to persons practically engaged in the business.

There are other shell-fish besides the oyster that will well repay the trouble of transplantation and multiplication. Among these are several species of clams belonging to the Pacific coast of the United States, which are much superior in size, in tenderness, and in excellence of flavour to those on the eastern coast. Most of these are natives of Puget Sound, and the completion of the Northern Pacific Railway is looked forward to as a convenient means of transferring them to Eastern waters. The common clams of the Atlantic coast are also fair subjects of experiment.

VOLCANIC DUST FROM NEW ZEALAND

A SHORT time since, Sir Julius von Haast sent me a small packet of volcanic dust from New Zealand, and requested me to examine it. The dust fell on June 10 of the present year (the day of the Tarawera eruption) at Matakava, Hicks Bay, 115 miles from the scene of the eruption. This dust is very fine, and, when regarded in the mass, is a dull, darkish gray colour. When examined under the microscope, it may be divided into—

(*a*) Bits of a more or less scoriaceous aspect—tiny lapilli, commonly almost opaque, being only translucent on thin edges—consisting of a somewhat brownish glass containing much disseminated ferrite. With reflected light they are a light to a darkish gray in colour, sometimes slightly reddish or brownish, with moderately rough surfaces. In size they usually vary from about ·005 to ·008 inch in the longest diameter; the former being the more common measurement; the latter is but rarely exceeded, the largest fragment in the portion which I have examined being ·012 inch in diameter; lapilli also occur of less than ·005 inch.

(*b*) Chips, more or less transparent, generally not exceeding ·005 inch in diameter, and of all dimensions downwards to the finest dust. The majority of these chips are glass, commonly quite colourless; some of them contain bubbles, spherical, spheroidal, or more or less cylindrical. Sometimes these are quite ·001 inch in longest diameter. Many chips show a ridgy surface, and are evidently formed by the destruction of a very frothy pumice like that of Krakatão. Some of the glass is of a light brown colour; occasionally it contains microliths of feldspar or trichites. The mineral chips are much less numerous than those of glass; the great majority of them are feldspar. Many of these are flat flakes apparently detached from a basal plane, but a few exhibit twinning. Some may be sanidine, but a plagioclastic feldspar is certainly present. The chips, however, are ill-suited for optical measurements, and the results which I have obtained are rather discordant. So far as I can come to a conclusion, I should say that the extinction-angles seem to indicate the not unfrequent presence of a feldspar which belongs rather to the oligoclase-albite group than to the labradorite-anorthite. I find very few indications of the presence of a pyroxenic constituent. One or two fragments are a greenish hornblende; three or four in general appearance resemble small flakes of magnesia-iron mica—lying on their basal faces, but some of these show dichroism, and only extinguish in certain positions between crossed Nicols, so that they cannot be this mineral. As to their true nature I have not yet been able to decide; however, I think it probable some of the brown flakes are mica.

The result of my examination leads me to conclude that the dust is formed of material which was a glass wherein a porphyritic structure, on a large or a minute scale, was inconspicuous. This Matakava dust appears to agree generally with, though it differs varietally from, that described by Prof. Joly in NATURE (vol. xxxiv. p. 595), the main difference being that the biotite, which seems rather common in his samples, is rare in, if not absent from, the present one. I have not noticed sulphur, pyrite, or magnetite in a recognisable form.

It may be interesting to compare this dust with some samples projected from Cotopaxi, and described by myself (*Proc. Roy. Soc.* No. 231). These specimens came from various distances, ranging from twenty to sixty-five miles from the volcano. That which fell on the summit of Chimborazo (the most distant locality) consists of lapilli and chips; the majority of the grains range from about ·001 to ·003 inch; a very few only attain to a diameter of ·01 inch, and this is barely exceeded. In this dust, however, the lapilli are comparatively rare, the chips of glass and mineral dominating, with a fair proportion of the latter. A reference to the above paper will show the difference between this ash, that from Krakatão, and the above-described from New Zealand. This may be explained by the fact that a porphyritic structure is common in the lavas of Cotopaxi (as in the other summits of the district).

T. G. BONNEY

NOTES

WE have to record the death of General John Theophilus Beaulieu, F.R.S., at the age of eighty-one years. He entered the Indian Army in 1820 as a lieutenant in the Bengal Engineers, and was for some time Superintending Engineer in the Public Works Department for the N.W. Provinces. Among other services to science and to India, General Beaulieu inaugurated the system of magnetic observations in India, and was the author of a book of logarithms of wide reputation. General Beaulieu was elected a Fellow of the Royal Society fifty years ago, and has served on its Council.

THE death is announced, at Berlin, of Dr. A. Fischer, so long resident at Zanzibar, and who has done so much for the exploration of the Kilimanjaro region.

M. CHANCOURTOIS, General Inspector of Mines in France, author of several works on geology, and Professor in the School of Mines, has died suddenly at Paris at the age of sixty-seven.

THE following are the probable arrangements for the meetings of the Society of Arts before Christmas:—"November 24, William Anderson, M.Inst.C.E., "Purification of Water by Agitation with Iron and by Sand Filtration." December 1, adjourned discussion on the paper by Dr. C. Meymott Tidy, on "Sewage Disposal" (read April 14, 1886). December 8, Major-General C. E. Webber, R.E., C.B., "Glow-Lamps, their Use and Manufacture." December 15, J. B. Marsh, "Cameo-cutting as an Occupation." There will be five courses of Cantor Lectures during the session:—(1) "Principles and Practice of Ornamental Design," by Lewis Foreman Day. (2) "Diseases of Plants, with special reference to Agriculture and Forestry," by T. L. W. Thudichum, M.D. (3) "Building Materials," by W. Y. Dent, F.C.S., F.I.C. (4) "Machines for Testing Materials, especially Iron and Steel," by Prof. W. C. Unwin. (5) "The Structure of Textile Fibres," by Dr. Frederick H. Bowman, F.L.S., F.G.S. Two Juvenile Lectures on "Soap Bubbles," by A. W. Reinold, F.R.S., will be given on Wednesday evenings, January 5 and 12, 1887. The meetings of the Colonial Section and of the Indian Section will not commence till after Christmas.

THE General Committee of the Society for the Prevention of Hydrophobia and Reform of the Dog Laws held its fourth meeting on Friday last at the offices, 50, Leicester Square, London, W.C., to consider a programme which had been drafted by a sub-committee and circulated among members and supporters. Mr. Victor Horsley, B.S., F.R.S., Secretary to the Commission on Hydrophobia, attended this meeting by invitation, and was unanimously elected a Member of Committee. Many letters were read expressing approval of the programme and regretting that the writers were unable to attend. Among the gentlemen who thus wrote were Dr. Drysdale, Prof. Fleming, Dr. Norman Kerr, Prof. E. Ray Lankester, Mr. Arthur Nicols, and Prof. Pritchard. The Honorary Secretary having made a financial statement of a satisfactory nature, the Chairman, Colonel R. H. Rosser, briefly explained the care and time given by the sub-committee to the programme, which was then discussed in detail, and ordered to be printed with some additions and alterations.

IT is intended, in Section III. of the Manchester Jubilee Exhibition, to exhibit the historical and modern methods adopted in the several branches of electro-metallurgy, such as gold, silver, platinum, nickel and copper plating, the purification of metals by electrolysis, and generally to illustrate the connection between electricity and chemistry.

INFORMATION has been received by the Board of Trade respecting the oyster fisheries of the Isle of Wight. The oyster grounds and breeding ponds of the Isle of Wight are as follows:—(1) Medina River; (2) Brading Harbour; (3) Newtown; (4) Fishhouse or Fishbourne. In 1867 the Isle of Wight Oyster Fishery Company was started, having the Medina River and Newtown Creek for its grounds. This oyster fishery is said to have done well until 1871, when it was troubled by refuse and sewage discharged by mills at Newport, and a large quantity of the broods were destroyed. The Medina fishery is now for sale. About the year 1873 Major Boyle started a system for breeding oysters at Brading, but in 1876 the harbour works were commenced, and Major Boyle had to relinquish his ponds. The Harbour Board, however, still carry on the oyster breeding at Brading. They have six or seven ponds near St. Helens, which are estimated to contain five or six millions of oysters at the present time. As Brading is not suitable for fattening, the young oysters are sold to various growers to be laid down for one or two years to render them fit for food. Newtown Creek is the best fattening ground in the Isle of Wight, but the fishery company is now in liquidation through want of funds, and the business is in abeyance. At Fishhouse there has been a good fall of spat this last summer, but the ground is much disturbed by barges, which prevents the fishery from being fully developed. The oyster fishery is consequently not very prosperous. The creek, it may be mentioned, was recently cleared, and some sixty or seventy thousand oysters were transferred to Newtown to fatten in a pond placed at their disposal by the Newtown Company. Besides the oyster fishery companies above referred to, there are several fishermen who dredge for oysters at sea in the Solent, and particularly in Osborne Bay. The oysters dredged up are seldom fit for the market, and have usually been sold to one of the companies to be laid down and fattened.

IN Dr. B. W. Richardson's recent Cantor Lectures on "Animal Mechanics," speaking of the mechanism of the heart, he described the number of the pulsations of the heart in different animals—in fish, frog, bird, rabbit, cat, dog, sheep, horse—and made a few comments on the remarkable slowness of the heart—40 strokes per minute—in the horse. Then the number of pulsations in man at various periods of life, and at different levels, from the level of the sea up to 4000 feet above sea-level, was brought under review, and was followed by a computation of the average work performed by the heart in a healthy adult man. The work was traced out by the minute, the hour, and the day, and was shown to equal the feat of raising 5 tons 4 cwt. one foot per hour, or 125 tons in twenty-four hours. The excess of this work under alcohol in varying quantities formed a corollary to the history of the work of the heart, Parkes's calculation showing an excess of 24 foot-tons from the imbibition of eight fluid ounces of alcohol. The facts relating to the work of the heart by the weight of work accomplished was supplemented by a new calculation, in which the course of calculation was explained by mileage. Presuming that the blood was thrown out of the heart at each pulsation in the proportion of 69 strokes per minute, and at the assumed force of 9 feet, the mileage of the blood through the body might be taken at 207 yards per minute, 7 miles per hour, 168 miles per day, 61,320 miles per year, or 5,150,880 miles in a lifetime of eighty-four years. The number of *beats* of the heart in the same long life would reach the grand total of 2,869,776,000.

A METEOR of unusual splendour was seen from the Oxford Road, Banbury, on Tuesday, November 2, at about 8.5 p.m. The fall became visible at about mid-distance between zenith and horizon in a direction west by north. At first the meteor burned with a faint, apparently reddish, light, but when a third of its path had been passed, it burst into an intense blue flame, and, increasing in brilliancy during the next third of its course, it finally died away before reaching the horizon, leaving behind it a long red trail distinguishable for several seconds afterwards. The fall was at an angle of 60°, and during the middle third of the flight the flame was of such intensity as to light up the surrounding country.

REFERRING to a paper at the Paris Geographical Society by Dr. Hamy on "The Interpretation of one of the Monuments at Copan, Honduras," in which an inference is drawn as to early Chinese intercourse with America, Dr. W. H. Dall, writing to *Science*, states his belief that the very wide hypothesis thus broached, and which in one form or another has had a certain currency for more than a century, rests upon a totally insufficient foundation. That wrecked Japanese, and possibly Chinese, from time to time were cast on the shores of America, is beyond question. But there is every reason to believe that the wrecked people were (1) nearly always males, and incapable of colonising; (2) were either killed or enslaved by the Americans in accordance with a general usage; and (3) that neither in arts nor language have they left any appreciable trace on American anthropology. "The statement of Brooks, that the Japanese and Aleuts could communicate without an interpreter, is true to this extent. I was present when the aforesaid Japanese, three males, were brought to the port of Unalashka, and took pains to inquire into the assertion which was made to me at the time. I found that the communications were wholly by signs, and not by spoken language, as the Aleuts could not understand a word of Japanese without its accompanying signs. Secondly, Brooks, who was long Consul in Japan, informed me that he had particularly searched into the matter of the voyage to Fu-sang, and that he had conclusive evidence that the voyage which actually took place was to the well-known and still existing province of Fu-sang in Korea (see Griffis' work), and had no connection whatever with America. Lastly, the mere presence of two simple curved lines on a circular stone, taken by itself, proves nothing as to their meaning, and still less that they had any connection with the Chinese symbol." Dr. Dall concludes by stating that such unbridled hypotheses are the "curse of anthropology."

WE have received a fresh instalment of the very valuable work emanating from the firm of Eduard Trewendt, Breslau, the now well-known "Encyclopædia of Natural Sciences," namely, Nos. 48 and 49 of the first part, and 37 and 38 of the second part. These four numbers deal with three different branches of science. No. 48 of the first part is botanical, a continuation of Drude's masterly work, "Systematic and Geographical Arrangement of the Phanerogams," copiously illustrated with woodcuts, as was the earlier portion of the same publication. No. 49 of the "Alphabetical Manual of Zoology, Anthropology, and Ethnology," takes the reader from "Landschaf" to "Lithodina." Of the abundant material embraced in this compass may be particularly mentioned "Landschnecken," by E. von Martens; "Larven," by Dr. Griesbach; two especially interesting contributions, "Larynx," by von Mojsisovics, and "La Tène-Zeit," by Mehlis; treatises by Jaeger on "Life, and its Conditions, Phenomena, and Stimulations, &c.," as also "Linsen," and "Lippen," by Dewitz. Nos. 18 and 19 of the second part belong to the "Alphabetical Manual of Chemistry," containing the papers—Furfurangruppe (conclusion), Gährung, Galle, Gallium, Gehirn, Gerberei, Gerbsäuren or Gerbstoffe, Germanium, Glas, Glycerin, Glycidsäuren, Glycocoll, Glycoside. As of most general interest in this list of articles may be specially cited "Glycoside," by Prof. Oskar Jacobsen, of which the firm is preparing a separate impression; as also "Gährung," by Tollens; and "Glas," by Engler. The newly-discovered "Germanium" is treated by the editor, Herr Ladenburg himself. In Prof. Liebrich's contribution, "Gehirn," we have a valuable paper on this subject by a recognised authority.

TWO remarkable specimens of deformed fish were taken from a rearing-pond at Delaford this week, and brought to the South Kensington Aquarium. One is a trout about three years old, whose tail is bent to such an extreme that it stands at right angles with the body of the fish. Its mode of progress is laboured, and its appearance is very peculiar. The other specimen is an ordinary stickleback, measuring 4 inches in length, whose body is swollen through dropsy to the extent of 1 inch in diameter. At first sight its appearance is similar to a young mouse, and it requires close inspection to grasp the fact that it is a fish. It moves very slowly, with great expenditure of force, the weight of the contorted body being considerable.

THERE has been a great demand for the German carp lately imported into this country by the Marquess of Exeter and others for the purpose of acclimatisation in this country, and a numerously expressed desire to stock waters that are useless for the purpose of maintaining other fish than carp has been made for the German species.

THE operations in connection with the new Observatory and Marine Fish-Culture Station at Lochbuie, Isle of Mull, to be established under the auspices of the National Fish-Culture Association, are to be commenced forthwith. At a meeting of the Council, held last week, it was arranged to form ponds for the propagation of lobsters, and to make certain observations upon marine fauna. The details of the scheme were placed in the hands of a special scientific committee.

WE have received from Messrs. Horne, Thornthwaite, and Wood, opticians, a map of the moon, 12 inches in diameter, mounted on good stout cardboard, represented as she appears when viewed through a telescope with an astronomical eye-piece. This will be a very useful companion and guide for those amateurs who are studying the lunar surface; it can be conveniently held in the hand while observations are being made. About 300 craters and walled plains are marked very clearly, and the names of the different seas are given in larger letters. At the back is added a short description of some of the chief features.

A SHOCK of earthquake was felt in the district of Beira Alta on the 11th inst.

THE additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas*), from West Africa, presented by Mr. Thomas Baily; a Yellow Baboon (*Cynocephalus babouin*), from West Africa, presented by Capt. J. Henderson Smith, R.A.; two Goshawks (*Astur palumbarius*), European, presented by the Baron d'Epremesnil; a Hobby (*Falco subbuteo*), caught in the Indian Ocean, presented by Dr. Rivis Mead; two Java Sparrows (*Padda orysivora*) from Java, four St. Helena Seed-Eaters (*Crithagra butyracea*), from South Africa, presented by Mrs. Conrad Pile; two Sing-sing Antelopes (*Cobus sing-sing* ♂ ♀), from West Africa, received in exchange; a Woodcock (*Scolopax rusticula*), European, purchased; an Ocelot (*Felis pardalis*) from America, a Bactrian Camel (*Camelus bactrianus* ♀) from Central Asia, two White-backed Piping Crows (*Gymnorhina leuconota*) from South Australia; a Banded Parrakeet (*Palæornis fasciatus* ♂), from India, deposited; a Vinaceous Turtle Dove (*Turtur vinaceus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE INFLUENCE OF ASTIGMATISM IN THE EYE ON ASTRONOMICAL OBSERVATIONS.—Prof. Seeliger has published, in the *Abhandlungen der k. bayer Akademie der Wiss.*, ii. Cl., xv. Bd., 3 Abth., an interesting paper on this subject. The paper is divided into four sections. The first part treats of certain details connected with the refraction of light which are used in the subsequent investigations. The second part gives the theory of the formation of images in an astigmatic eye, and its application to measures made with an altitude instrument. In the third and fourth parts the author treats of the application of his theory to the heliometer and wire-micrometer respectively. It appears, from Prof. Seeliger's researches, that this malformation in the eye, which is far from uncommon, exerts a larger influence on astronomical measurements than is commonly supposed. Thus, he shows that a systematic error in a series of observed declinations amounting to 0″·26 may very well be due to it. And it appears that the discordances in observed position-angles of double stars, depending on the inclination of the line joining the components to the vertical, with which the measures of some observers are affected, may in part be referred to the same cause. Prof. Seeliger's paper is one which may be profitably studied by those who aspire to the attainment of greater accuracy in astronomical observations.

THE KALOCSA OBSERVATORY.—Dr. C. Braun has recently published a report of this Observatory, founded by Cardinal Haynald, Archbishop of Kalocsa. The equipment of the Observatory consists of a refractor, by Merz, of 7 inches aperture; another of 4 inches; a transit by Cooke, aperture 2·3 inches; an altazimuth by Breithaupt, of Cassel; a chronograph, three clocks, and a chronometer; several spectroscopes, of which a large solar spectroscope with automatic adjustment to minimum deviation is the principal; a star photometer by Zöllner, and a spectro photometer by Vogel and Glan. The two most important works effected at the Observatory have been the determination of the geographical position of the Observatory, and the observation of sunspots. A special value attaches to the former, as hitherto the position of no place in Hungary had been fixed by direct astronomical methods. The latitude of the standard pillar of the Observatory was determined by geodetic observations to be 46° 31′ 41″·92; the astronomical methods made it 0″·07 greater. The longitude was found to be 1h. 15m. 54·343s. east of Greenwich. The observations of sunspots extend from May 14, 1880, to January 31, 1884, and form a useful record of an interesting period. The method of projection was employed in observing; the observations were reduced first by means of a projection of the sun, and secondly by calculation. In the latter method Dr. Braun employed an instrument of his own device, which he terms a trigonometer, for the direct solution of spherical triangles. From his observations Dr. Braun deduces the following expression for the velocity of rotation:—$865^{\circ}\cdot 33 - 209^{\circ}\cdot 86 \sin^2 \lambda$. He also shows the downward tendency in latitude of the mean spotted area, and points out the curious partial effort at recovery which shows itself at tolerably regular intervals. The observations of each rotation are grouped together and given in short tables, and diagrams similar to Carrington's, showing the spots of each rotation in shape and position, are also added. The volume concludes with full descriptions of a number of ingenious instrumental devices, some actually employed at Kalocsa, others still only projected. Amongst these is an ingenious transit micrometer for eliminating personal equation in the observation of transits.

ζ CASSIOPEIÆ.—The *Sidereal Messenger* reports, on the authority of Prof. Colbert of Chicago, that this star appeared to increase its brilliancy by quite half a magnitude on the night of August 20. The most remarkable point of the observation was the shortness of duration of the phenomenon: for, about half an hour after it was first noticed, the star began to return to its normal magnitude. It will be interesting to learn if the change was observed elsewhere.

NEW MINOR PLANETS.—Prof. Peters discovered a new minor planet, No. 261, on October 31, and Herr Palisa two—Nos. 262 and 263—on November 3.

COMET FINLAY.—The following ephemeris for Berlin midnight is in continuation of that given in NATURE for November 4 (p. 17):—

1886	R.A. h.	m.	s.	Decl. °	′		log *r*	log Δ
Nov. 16	19	59	51	23	22·7	S.	0·0697	0·0899
18	20	8	42	22	53·7			
20	20	17	38	22	22·4		0·0689	0·0874
22	20	26	37	21	48·8			
24	20	35	39	21	13·0	S.	0·0693	0·0856

COMET BARNARD.—The following ephemeris for Berlin midnight is given by Dr. E. Lamp (*Astr. Nachr.*, No. 2753):—

1886	R.A. h.	m.	s.	Decl. °	′		log *r*	log Δ	Brightness
Nov. 18	13	16	59	13	12·4	N.	9·9433	0·0637	10·8
20	13	31	51	14	5·9		9·9306	0·0485	12·3
22	13	47	57	14	58·3		9·9180	0·0340	13·9
24	14	5	20	15	47·7		9·9055	0·0207	15·7
26	14	24	2	16	32·5		9·8934	0·0089	17·5
28	14	43	57	17	10·3		9·8817	9·9990	19·3

The brightness at the time of discovery is taken as unity.

GOULD'S "ASTRONOMICAL JOURNAL."—The first number of the new issue of this journal appeared on November 2. It contains the following papers:—On the light-variations of Sawyer's variable in Vulpecula, by S. C. Chandler, Jun., in which the elements of the star are given as Max. = 1885 Nov. 2d. 20h. 35m. G.M.T + (4d. 10h. 29·0m.) E. The minimum is 1·060d. earlier. The rapidity of the rise is a striking characteristic of this star.—A new short-period variable, by E. F. Sawyer. The star, 57 Sagittarii, has a period of not more than 6 days; the variation is from 5·6 mag. to 6·6. Place for 1875·0, R.A. 18h. 14m. 2s.; Decl. 18° 54′·8.—Elements and ephemerides and observations of Comets Finlay and Barnard, by Profs. Winlock, Boss, and Frisby.—Observations of U Ophiuchi, by E. F. Sawyer; and the first part of a paper on the lunar theory, by Prof. Stockwell.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *NOVEMBER* 21-27

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 21

Sun rises, 7h. 31m.; souths, 11h. 46m. 3·1s.; sets, 16h. 1m.; decl. on meridian, 19° 58′ S.: Sidereal Time at Sunset, 20h. 3m.

Moon (New on November 25) rises, 1h. 37m.; souths, 8h. 4m.; sets, 14h. 18m.; decl. on meridian, 1° 57′ N.

Planet		Rises h.	m.		Souths h.	m.		Sets h.	m.		Decl. on meridian °	′	
Mercury	...	9	30	...	13	11	...	16	52	...	25	2	S.
Venus ...	...	7	12	...	11	35	...	15	58	...	18	46	S.
Mars ...	...	10	33	...	14	18	...	18	3	...	24	32	S.
Jupiter...	...	4	15	...	9	34	...	14	53	...	8	45	S.
Saturn...	...	19	34*	...	3	36	...	11	38	...	21	22	N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Nov.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image	
			h. m.	h. m.	°	°
22	46 Virginis	6	4 42	5 31	72	176
22	*k* Virginis	6	4 58	near approach	304	—
22	48 Virginis	6	6 36	7 8	104	163
23	B.A.C. 4647	6	4 42	5 40	24	222

Nov.	h.	
23	1	Jupiter in conjunction with and 3° 0′ south of the Moon.
23	14	Mercury stationary.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Nov. 28,	2 27	*m*
ζ Geminorum	6 57·4	20 44 N.	,, 23,	21 30	*M*
S Canis Minoris	7 26·5	8 34 N.	,, 24,		*M*
S Sagittarii	19 12·8	19 14 S.	,, 21,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 24,	0 0	*M*

M signifies maximum; *m* minimum.

Meteor Showers

The *Andromedes*, maximum November 27, R.A. 24°, Decl. 44° N., form the most interesting shower of the week. A radiant near μ Persei, R.A. 60°, Decl. 49° N., supplies very swift meteors; swift meteors are likewise seen from a radiant near η Ursæ Majoris, R.A. 208°, Decl. 43° N.

GEOGRAPHICAL NOTES

THE *Bulletin* of the Paris Geographical Society for the present year (Nos. 1 and 2) contain several papers of interest. We need do no more than mention M. Ch. Mannoir's annual report on the progress of geography during the past year, which fills 130 pages. M. Grandidier writes on the rivers and lagoons of part of the east coast of Madagascar, and M. Gouin, of Nam-dinh, contributes a long paper on Tonquin, which deals with the commercial geography of the country rather than with the geography proper. No. 2 opens with the report of a strong Committee of the Society on the orthography of geographical names, which will be read with interest. No elaborate or exhaustive reforms are proposed; the suggestions are rather a series of simple modifications "based on good sense rather than on high philological science, which is only accessible to the few initiated." The Committee take up the programme of the Royal Geographical Society, "completing it in some respects, and making some additions sensible to musical ears." The bases of the proposals are the same as those of our own Society: (1) not to seek an absolute perfection in the representation of sounds; (2) to preserve in European names the form of the country of their origin; (3) to retain in the case of other places the mode commonly employed. M. Rolland contributes a long paper on the hydrography and orography of the Algerian Sahara; and M. Marteil examines the map of the French establishments on the Senegal recently issued by the Ministry of Marine. Lieut. Baudens describes a trip which he made last year along the Black River of Tonquin; and finally there is an account written by Dr. Potagos in 1880 of a journey which he made in the Pamir in 1870, including a visit to the famous Yakub Beg of Kashgar.

WE have received the *Verhandlungen* (Bd. xiii. No. 8) and the *Zeitschrift* (Bd. xxi. Nos. 4 and 5) of the Geographical Society of Berlin. The first contains only one paper, but that an interesting one, by Dr. Sievers, on a recent journey in the Sierra Nevada de Santa Marta, in Columbia. The object of the journey was to study the geology and physical geography of the region, and especially to ascertain whether these mountains belong to the system of the Andes or not. As Dr. Sievers has only been back for a short time, he was unable to give any definite results, and he confines himself to describing the course of the journey, and to mentioning important points necessary for a proper understanding of the physical geography of the region. In the *Zeitschrift*, Herr Jung continues and concludes his analysis of the Indian census of 1881; this is followed by a translation, from the *Proceedings* of the Russian Geographical Society, of Dr. Iwanow's paper on certain ancient monuments discovered by him in the course of a geological examination of Turkestan. Prof. Gelcich has a highly technical history of the methods of ascertaining the area of a country, and Dr. von Danckelmann one on the frequency of rain in the Indian Ocean. Herr Sandler makes a contribution to the history of cartography by giving an account of the life and works of Johann Baptista Homann, a geographer of the latter part of the seventeenth century. A curious map appended to this paper (which is of considerable length) shows, by means of white and red outlines, the world according to present cartography and according to Homann's maps. The number concludes with a short paper on the hot springs of Kamchatka.

IN a recent work on the geology and geography of Sumatra, M. Verbeek, a Dutch engineer, says that sixty-seven volcanoes are known to exist on that island. There may be more even than this, for parts of the north-west, which are covered with primæval forests, have never been penetrated. Two only of these are active, Merapi and Talang (or Soclau), the former being 2892, and the latter 2542 metres in height.

THE October issue of the *Bollettino* of the Italian Geographical Society contains an account of an excursion made during the summer by E. Modigliani to the rarely visited island of Nias, which lies some thirty miles from the west coast of Sumatra, a little north of the equator. The explorer spent two months in the place, but owing to local feuds was unable to penetrate beyond Fadoro, a large village near Telok Dalam Bay on the south side. The natives, apparently of Malayan or Indonesian stock, but speaking a language quite different from Malay, and by Crawfurd described as "a simple, mild, and primitive people," he found on the contrary to be fierce and treacherous savages, everywhere addicted to head-hunting. Their hostility was such, that he failed to make any botanical or zoological collections; but fortunately secured eleven human skulls from the southern districts, which have been sent to the Anthropological Museum of Florence. No similar specimens appear to have hitherto reached Europe, nor are any found even in the Batavian collection. Head-hunting is taken so much as a matter of course, that on Sig. Modigliani offering to purchase some skulls, the rajah of Bavolovalani on the south coast quietly remarked that it would be rather an expensive business, as an expedition would have to be specially fitted out and sent to the hills to raid upon some neighbouring tribes and carry off the required number of heads. He had no idea of craniological specimens being collected except from the living subject. The interior of Nias still presents a promising field of exploration, never having been visited by any European travellers.

LIGHTHOUSE ILLUMINANTS[1]

II.

V.—*Range of Lights in Hazy Weather*

THE observations on this subject of the Trinity House Committee have served to confirm the conclusions announced by M. Allard in his "Mémoire sur l'intensité et la portée des phares," 1876, and in his more recent "Notes sur quelques objections relatives à l'emploi de la lumière électrique dans les phares." The Committee find that the gas and oil lights which are equal in clear weather are equal also in fogs; that in rather dense fog the more powerful light had but little advantage over the less powerful, for example, "the triform electric appearing at 1500 feet, while the quadriform gas and triform oil showed up together a little before the observers reached 1400 feet," and that the electric light, while suffering, according to the photometric results, a somewhat greater loss in hazy weather than the flame lights, is "visible at a greater distance than the highest powers tried in gas or oil." Using M. Allard's formula, which appears to rest on well-established physical and physiological data, I have calculated the range in fogs of various degrees of thickness of some of the lights exhibited at the South Foreland. The range of a light, or the limit at which it is just lost or just picked up, is that limit at which its intensity is diminished by distance and haze to the minimum intensity perceptible by a good eye, such as the practised eyes of seamen are. M. Allard gives this minimum intensity, on the authority, of "des expériences qui ont été faites sur ce sujet au Champ de Mars," as that of 1/100 Carcel at a distance of one kilometre on a perfectly clear night. This corresponds to ⅛ candle at a distance of one nautical mile. When the air is not perfectly clear, its degree of transparency may be expressed by stating the fraction of light which escapes obstruction in passing through a certain length. Of this fraction the same fraction escapes obstruction in passing through another equal length of air, and so forth. Thus, if this fraction is called a, and l is the intensity at any point of a beam of parallel rays, such as a beam of sunlight reflected from a plane mirror, after the beam has traversed a mile of hazy air its intensity is diminished to la, at two miles its intensity is diminished to la^2, and at a distance of d miles to la^d. But divergent light, such as is even the most condensed beam from a lighthouse, diminishes also as the square of the distance. Thus, if L is a lighthouse light whose intensity at one mile is La, its intensity at any number of miles, d, is $La^d \times \frac{1}{d^2}$, and when the combined effect of haze and distance is such that its intensity is only equal to that of ⅓ candle at one mile, at that point the light ceases to be visible. Thus it is possible to calculate for any particular degree of haze what will be the range of any given light. To give some examples:—In a moderate uniform haze such that a single 108-jet gas-burner, showing as a fixed light of about 14,000 candles, was lost at a distance of 10·9 miles, the same light shown in biform would be lost at 11·8 miles, while the corresponding triform and quadriform lights would be lost at 12·5 and 12·8 miles respectively. In a rather thicker haze, in which a single 108-jet gas-burner, showing as a revolving light of 60,000 candles, was visible up to 10 miles, but no further, the extreme range of the biform would be 10·73 miles, of the triform 11·16 miles, of the quadriform 11·48 miles. In still thicker haze the increase of range obtained by increasing the power of the lighthouse light becomes not only absolutely but relatively less.

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[1] Further Report of Mr. Vernon Harcourt to the Board of Trade on the Experimental Lights exhibited at the South Foreland. Continued from p. 46.

Light shown	Range in Nautical Miles		
Single 108-jet, M. I. lens	2	1	0·5
Biform ,, ,,	2·11	1·05	0·52
Triform ,, ,,	2·17	1·08	0·54
Quadriform ,, ,,	2·22	1·1	0·55

The above results represent the maximum range of the direct beam through uniform haze of lights of the same kind but varying in power. But in certain cases the increase of range gained by increasing the power of the light may be either less or greater than it is in the foregoing case.

In the first place, the light which has suffered obstruction is diverted from its direct course but is not lost; and a portion of this light may reach the eye from a direction slightly different from that of the source of light, producing the impression of a halo or burr. Prof. Stokes, Pres. R.S., who has kindly given me much help in considering this subject, concludes that, especially in a fog in which the particles of water are not very minute, the burr might be seen at a substantially greater distance than that at which the direct light could be seen. "The intensity of this diffused light will not decrease in geometric progression as the distance from the source increases, but rather will tend ultimately to decrease inversely as the square of the distance; but being so widely spread, there will be danger of its being unperceived unless it be flashy." In this case the more powerful lights would retain in fog more nearly the advantage which they possess in clear weather, in which a fourfold light has double the range of a single light. But since the eye can distinguish between the point of light which is seen by the direct rays and the blurred nimbus, whose properties Prof. Stokes has investigated, the question whether the range of powerful lights is materially increased by the diverted and re-diverted light which surrounds the principal beam could be solved by an appeal to experience.

In answer to my inquiry whether it often happens that in approaching a lighthouse on a hazy night that which is first seen is an indistinct brightness or halo, not the light itself, and whether this effect is seen at considerable distances or only at short distances, and in what kind of fog, the Deputy Master of the Trinity House tells me that it happens, occasionally, at short ranges, in thick fog or mist, when nothing of the light is seen beyond 1 or 2 miles. "In clearer weather (*i.e.*, slight haze) this peculiarity is not observable at any range; it is the direct beam from the lantern (of course lessened and indistinct by reason of the density of the atmosphere) which then comes to the eye of the observer when approaching from seaward." I think, therefore, it may be concluded that at short distances powerful lights may occasionally have an advantage over feebler lights greater than is indicated by M. Allard's formula, in consequence of the scattered light being less diminished by fog than the direct light.

There are two other cases in which the formula is not immediately applicable; the second exception being, if the haze is not uniform. This case may be illustrated by taking an extreme example. Suppose that the amount of fog extending 5 miles from a lighthouse were just sufficient to extinguish a light of 60,000 candles, and that beyond this distance the air were perfectly clear, a light of four times the initial power would have at the margin of the fog four times the minimum visible brightness, and would only disappear altogether at a distance of 10 miles. But, on the other hand, if the fog were thicker further to seaward, the larger light would have scarcely any advantage over the smaller light.

Thirdly, if the lights compared differ in quality, of which the visible sign is colour, as well as in power. In this case the particles of water of which haze at sea consists (differing from the coloured particles of a London fog) are only likely to exercise a selective action on lights of different refrangibilities when the particles are so small as to be comparable with a wave of light. In a thick mist, in which the particles of water have often a visible magnitude, this effect is probably absent. Clouds are of this character, and sunlight is not reddened by passing through them. But the red colour of the sun when near the horizon, and the assimilation in colour of the electric light to gas-light when seen from a distance through slight atmospheric haze, shows that such haze does interfere with the more refrangible blue rays, to a greater extent than it does with the yellow, orange, and red rays. It is therefore certain that the electric light, which contains a relatively large proportion of the more refrangible rays, suffers a greater loss than the light from gas or oil flames in certain states of the atmosphere. The larger particles of mist or rain probably obstruct light of all sorts in the same degree. If we suppose that the effect of haze is to cut off all the blue and violet rays, the loss to the flame lights would not exceed 1 or 2 per cent., while that of the electric light, which is perhaps rather bluer than sunlight, may amount to 20 per cent. But this loss, though considerable, would not materially affect the range of the electric light in hazy weather.[1]

It has been claimed as an advantage of multiform lights, compared with the electric arc behind a small lens, that the larger surface of illuminated lens is more favourable to visibility in hazy weather; and the late Sir W. Siemens gave some countenance to this view. Speaking in the discussion of Sir James Douglass's paper "On the electric light applied to lighthouse illumination," 1879, he said:—"He had held that, in order to get more penetrating power, not intensity alone, but intensity with quantity as represented by large surface, would be required." M. Allard in his "Note," pp. 10 and 11, makes some interesting observations on this subject, but deals rather with visibility at great distances in clear weather than with visibility through haze. I do not know on what grounds, either of theory or observation, the opinion formed by Sir W. Siemens is based. Desiring further information on this and some other points, I have consulted Lord Rayleigh, Sec. R.S., who with Prof. Stokes joined me in a visit to the South Foreland lights a year ago. I may quote his opinion:—"With the same total brightness of source, and angle of divergence, it can make no difference at a distance (at which the apparent magnitude of the lens is inappreciable), whether the lens be large or small. At smaller distances the advantage might be with the smaller lens. So far as I see, the only advantage that the large lens could ever have would be more room for a bulky light, which, with a small lens, might give too great a divergence."

While referring to the assistance I have received from Prof. Stokes and Lord Rayleigh, to whom I desire to accord my thanks, I should mention that I have received from both the same emphatic suggestion, that further trial should be made of sudden flashes in fog. Lord Rayleigh writes: "I should like to see proper experiments tried on sudden and periodic flashes, such as might be produced by gunpowder, the periodicity serving for identification and the intermittence being necessary to get the necessary contrast, which is here between appearances at consecutive *times*, instead of as in ordinary vision between appearances in neighbouring *directions*."

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[1] Since this report was written, a paper has been communicated to the Royal Society by Captain Abney, R.E., F.R.S., and Major-General Festing, R.E., F.R.S., giving the result of measurements of the illuminating power of different parts of the spectrum of the electric arc. According to these measurements, the illuminating power of that part of the light from the electric arc which lies beyond the line "E" in the spectrum, including the greater part of the green rays as well as the blue and violet rays, is rather less than one-sixth of the total illuminating power.

Prof. Stokes writes, on various occasions:—"The diffused light of a powerful lamp would be weak and perfectly steady, and might thus escape notice, while a diffused light, even though no stronger, if almost momentary like lightning might be sufficient to attract attention, and you could afford to throw great chemical force into the formation of a flash which was to last only, as it were, for a moment." "If there are to be any more experiments, I think it would be well worth while to try explosions." "The more I think of it the more disposed I am to think that it would be worth while to try some experiments with flashes, I mean with explosions. The plant would not be at all costly; in fact, it would hardly cost anything. The chemicals would not cost very much. Preliminary experiments on a small scale, which could be tried anywhere, would show what chemicals were good to use in order to get a flash as bright as may conveniently be. But when the actual experiments in fog are tried, the quantity used should not be by any means very small. It should be enough to make the quantity of light for the moment much greater than what was kept up in your most powerful burners; but that would not require a very large quantity."

I should add that Prof. Stokes is aware that "flares" have been tried and used, but is clearly of opinion that further trials should be made in that direction. As to the great effect of sudden illumination in attracting attention there can be no doubt; but I find it hard to believe that in a fog in which an observer at a small distance was unconscious when the beam from the electric arc, through such a lens as the Mew Island lenses, passed quickly across him, any "flare" would be perceptible at the same distance. I do not know whether the experiment has been tried of combining a light-and-sound signal, by placing the materials used to produce a flash together with a charge of compressed gun-cotton in the head of a rocket, and so firing the rocket that it would explode at a fixed distance and bearing from the station and at a moderate altitude. A rocket will go through any fog, and might be used to give a lift seaward to the light and sound.

VI.—*Cost of each System*

I have not the data which are necessary for forming an independent opinion upon the estimates furnished in the Trinity House report. These estimates rest on unexceptionable authority; and I only venture to make any remarks on the subject because I am not entirely satisfied with the position assigned to the electric light.

I gather from the reference to the photometric results of Mr. Valentin that in a former estimate the cost was considered in relation to the yield of light by each illuminant. And I think that an estimate of what light can be had on any system for a given expenditure, or what the cost is on any system of a desired quantity of light, is needed to make the comparison of the relative cost complete. For example, a comparison is made in the Report (Tables I. and II., pp. 62–63), between a "first order oil-lighted tower for one six-wick burner only," and a "gas station to show a quadriform light of four times 68-jet power." The annual cost of maintenance, including interest on capital outlay, is estimated at 724*l.* for the first, and 1687*l.* for the second. But the light from a six-wick burner shown as a fixed light is probably rather less than 7000 candles, while that from the 68-jet quadriform is 35,000 candles. Thus, if such a quadriform were substituted for or adopted instead of the single light, both the cost and the light would be largely increased, but the increase of light would be more than double the increase of cost. Comparing the two as revolving lights, the increase of light would be one-third greater than the increase of cost. So in the comparison between the cost of the electric light and the other illuminants. The expense of annual maintenance, with interest on capital outlay, is estimated as being one-seventh greater for the electric light than for the quadriform 68-jet gas; but if the yield of light is taken into account, the figures appear very differently. The electric light is ten times as powerful as the quadriform. The actual cost of light, which is the commodity produced, may be compared in the two cases by stating the cost of maintaining for one year each 1000 candles intensity of light in the beam sent forth. By the electric arc the cost is about 1*l.*, by the quadriform gas 8*l.* 10*s.*

As to the actual estimate of the cost of the electric light, I venture to suggest that some reduction may be found practicable. Two engines of 30 horse-power, at a cost of 1250*l.*, seems a very full provision for the unfailing maintenance of one arc light. The actual horse-power absorbed by one of the De Méritens machines is given by Prof. Adams as 10·4. M. Allard states the price of one of M. de Méritens' machines, tested and recommended by him in 1880, as 350*l.* Dynamo-machines are, I believe, less costly. I think all that is needful and best of machines, lamps, and cables, may now be obtainable for less than 2154*l.*; and the estimate of 1217*l.* a year for wages, clothing, coke, oil, carbons, &c., and repairs and renewals, may perhaps be found in excess of the necessary cost. M. Allard gives the following details of the cost of the apparatus and expenses at Grisnez:—

	£
Two steam-engines of from five to six horse-power ...	488
Two magneto-electric machines	852
Four lamps	240
Total	1580
Annual expenses	537

He also furnishes an estimate, which I transcribe, of the average cost of converting an oil lighthouse into an electric lighthouse:—

	£
Engine-house, keepers' dwellings, water-supply, &c. ...	1600
Two steam engines, of from six to eight horse-power, with shafting and straps	640
Two electric machines with cables, &c.	560
Four lamps	240
First order lantern with oblique framing	800
Optical apparatus, two feet in diameter, with arrangements for revolving light	520
Sundries, packing, carriage, and setting up	240
Contingencies	400
Total	5000

Add "First cost of an ordinary first order oil-lighted tower" (T. H. Report, Part I., p. 75), less "Apparatus, lantern and glass," viz. 4834*l.*; and it would seem to follow that the first cost of an electric light station need not exceed 10,000*l.*

The rather large difference between this estimate and that of 17,749*l.* furnished by the Trinity House Committee has no doubt already received the consideration of the Elder Brethren, who have had the details of M. Allard's scheme before them.

In order to obtain some further information, I have inquired of a London firm of electric light contractors as to the cost of such an installation, and have received the following figures:—

	£
Two 30 horse-power *effective* engines	700
Three lamps, necessary cable, and labour	400
Two dynamos and spare armature to fit either ...	480

It will be seen that these figures are not very different from those of M. Allard's estimate.

In concluding this report I desire to thank the Elder Brethren of the Trinity House, and especially the Members of the Committee, for the kindness which has made the duty of co-operating with them in this inquiry a continual pleasure.

March 9, 1886 A. VERNON HARCOURT

Letter to the Board of Trade from the Commissioners of Northern Lighthouses

Northern Lighthouse Board, 84, *George Street*,
Edinburgh, *December* 10, 1885

SIR,—I am directed by the Commissioners of Northern Lighthouses to acknowledge the receipt of your letter of the 3rd instant, in which you request to know whether the Commissioners propose to make any general report or observations relative to the recent Lighthouse Illuminants Experiments at the South Foreland.

In reply I am directed to state that in view of the very able and exhaustive Report (Part I.) by the Committee of the Elder Brethren on this subject, in the conclusions of which the Commissioners concur, and of the report by their engineers dated October 14 last, which accompanied my letter to the Elder Brethren of the following day, and copy of which is herewith transmitted for the information of the Board of Trade, the

Commissioners do not propose, at least in the meantime, to make any general report or further observations in regard to these experiments.—I am, &c.,

(Signed) J. M. Duncan
The Assistant Secretary, Harbour Department,
Board of Trade

Report on Part I. of Report by the Special Committee of the Elder Brethren on Lighthouse Illuminants, by T. and D. Stevenson, Engineers, to the Northern Lighthouse Board

On September 2 last, the Board remitted to us, for consideration and report, Part I. of the Report of the Committee of the Trinity House, on the experiments at South Foreland, and having perused the reports of the experiments, we now beg to report as follows:—

As the Board is aware, the object of these experiments was to determine the relative merits of gas, oil, and electricity, as lighthouse illuminants, especially as regards their penetrative power in fog; also to ascertain the merits of certain optical arrangements, and test certain improved oil and gas burners patented by Sir James Douglass and Mr. Wigham. We think that these experiments embrace all the suggestions that were brought under the notice of the Elder Brethren for trial by the Board of Trade, and which led that Board to suggest that this train of experiments should be entered upon.

We have had several opportunities of inspecting the various kinds of apparatus, and also of witnessing the experiments, and in our opinion these experiments have been of an exhaustive character, and have been conducted with great care, and, we believe, in a spirit of the most perfect fairness and impartiality to all parties; and we have further to express our concurrence in the conclusions of the Committee as expressed in the following terms:—

"(1) That the electric light, as exhibited in the A experimental tower at South Foreland, has proved to be the most powerful light under all conditions of weather, and to have the greatest penetrative power in fog.

"(2) That for all practical purposes the gaslight, as exemplified by Mr. Wigham's multiform system in B experimental tower, and the oil light, as exemplified by the Trinity House Douglass six-wick burners in multiform arrangement up to triform in C experimental tower, when shown through revolving lenses, are equal, light for light, in all conditions of weather; but that quadriform gas is a little better than triform oil.

"(3) That when shown through fixed lenses, as arranged in the experimental towers, the superiority of the superposed gas light is unquestionable. The larger diameter of the gas flames and the lights being much nearer to each other in the gas lantern, give the beam a more compact and intense appearance than that issuing from the more widely separated oil burners.

"(4) That for lighthouse illumination with gas, the Douglass patent gas-burners are much more efficient and economical than the Wigham gas-burners.

"(5) That for the ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

We may explain that so far back as 1869 we had also occasion to conduct a series of experiments at Edinburgh on certain large burners patented by Mr. Wigham, and brought before the Board, with the view of increasing the power of sea lights, and again in 1870 a further series of experiments was carried out by us, by the direction of the Board of Trade, on the merits of the electric light, and certain important results were obtained during these sets of experiments at Edinburgh. The general result of the gas experiments then made was that the large 52-jet gas-burner was in no way superior to the 4-wick oil-burners when used in connection with the ordinary annular lens, as the "greater portion of the 7-inch gas flame" was with that size of lens necessarily ex-focal. The large burner was slightly superior, however, when used in a fixed light apparatus. Our opinion, which has been corroborated by the recent experiments, therefore, was that in order to bring out the full power of these large flames, an apparatus of larger focal distance than usual must be employed, and hence we designed the lens of 1,330^{mm} radius, which has recently been tried at the South Foreland with the most satisfactory results.

The electric light experiments made at Edinburgh in 1867–1869, showed that if the electric light beam was made to diverge artificially to the same extent as the 4-wick oil flame, it was in no way superior in brilliancy, and pointed to the advisability of adopting for the electric light the azimuthal condensing system of Mr. Thomas Stevenson. This system has been adopted for the electric light apparatus at the South Foreland experiments. The Edinburgh experiments further showed that it was necessary in any test of the intrinsic merits of electricity, gas, or oil, that the maximum condensation consistent with the requirements of navigation for each should be employed. At the South Foreland experiments, however, the condensation of the electric light was only 30° into 5°, that is only 6 times, whereas the Isle of May apparatus, which was exhibited at the South Foreland, condensed 45° into 3°, that is 15 times. Had this or a still more condensed light been used in the experiments, the electric light would have shown even greater superiority than it did.

We shall afterwards report as to what, in our opinion, is the arrangement of optical apparatus best suited for each illuminant. (Signed) T. and D. Stevenson

Edinburgh, October 14, 1885

OFFICIAL REPORT ON THE USE OF OIL AT SEA FOR MODIFYING THE EFFECT OF BREAKING WAVES[1]

THE following Memorandum, dated June 16 last, on the use of oil at sea for modifying the effect of breaking waves, has recently been printed and circulated by the Admiralty:—

"Many further practical experiments at sea have been made since the report by Capt. Chetwynd, R.N., to the Royal National Lifeboat Institution, dated September 30, 1884, on the use of oil for smoothing broken or troubled waters, which report was communicated to Commanders-in-Chief in Admiralty Circular Letter of December 1, 1884, N.S. $\frac{3206}{8305}$.

"As these further experiences go to show that the use of oil, under different circumstances, is of very extended and simple application, my Lords Commissioners of the Admiralty consider it desirable, in order that the facts may be generally known, to re-issue the report above mentioned, together with such other information as may serve for the guidance of officers, whose attention is hereby called to the fact that a very small quantity of oil skilfully applied may prevent much damage both to ships (especially the smaller classes) and to boats by modifying the action of breaking seas.

"The principal facts as to the use of oil are as follow:—

"On free waves, *i.e.* waves in deep water, the effect is greatest.

"In a surf, or waves breaking on a bar, where a mass of liquid is in actual motion in shallow water, the effect of the oil is uncertain, as nothing can prevent the larger waves from breaking under such circumstances; but even here it is of some service.

"The heaviest and thickest oils are the most effectual: refined kerosene is of little use; crude petroleum is serviceable when nothing else is obtainable; but all animal and vegetable oils, such as waste oil from the engines, have great effect.

"A small quantity of oil suffices, if applied in such a manner as to spread to windward.

"It is useful in a ship or boat, both when running, or lying-to, or in wearing.

"No experiences are related of its use when hoisting a boat up in a seaway at sea, but it is highly probable that much time and injury to the boat would be saved by its application on such occasions.

"In cold water, the oil being thickened by the lower temperature, and not being able to spread freely, will have its effect much reduced. This will vary with the description of oil used.

"The best method of application in a ship at sea appears to be hanging over the side, in such a manner as to be in the water, small canvas bags capable of holding from one to two gallons of oil, such bags being pricked with a sail needle to facilitate leakage of the oil.

"The position of these bags should vary with the circumstances. Running before the wind, they should be hung on either bow, *e.g.* from the cathead, and allowed to tow in the water.

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[1] From the *Board of Trade Journal*.

"With the wind on the quarter, the effect seems to be less than in any other position, as the oil goes astern while the waves come up on the quarter.

"Lying-to, the weather bow and another position farther aft seem the best places from which to hang the bags, with a sufficient length of line to permit them to draw to windward while the ship drifts.

"Crossing a bar with a flood tide, oil poured overboard and allowed to float in ahead of the boat, which would follow with a bag towing astern, would appear to be the best plan. As before remarked, under these circumstances the effect cannot be so much trusted.

"On a bar with the ebb tide, it would seem to be useless to try oil for the purpose of entering.

"For boarding a wreck, it is recommended to pour oil overboard to windward of her before going alongside. The effect in this case must greatly depend upon the set of the current and the circumstances of the depth of water.

"For a boat riding in bad weather from a sea-anchor, it is recommended to fasten the bag to an endless line rove through a block on the sea-anchor, by which means the oil is diffused well ahead of the boat, and the bag can be readily hauled on board for refilling if necessary."

ON THE INTENSITY OF REFLECTION FROM GLASS AND OTHER SURFACES[1]

THE author pointed out that most previous experimenters, especially Rood, had measured the amount of the transmitted light, and that any percentage of error in this measurement was greatly multiplied when the results were used to calculate the amount of reflected light. In his experiments the amount of reflected light was measured directly. The method was as follows. Light from a cloud was passed through ground glass in the window of a darkened room, and made to fall at the polarising angle on a plate of glass. The transmitted and reflected rays were conducted along different paths by a series of reflectors, but finally emerged side by side and of equal intensity. One of the reflectors in the path of the reflected ray was the glass surface to be tested, the light falling on it at an almost perpendicular incidence. This glass was now removed, and a single mirror was shifted so as to make the angles and points of incidence of the reflected ray on the several mirrors the same as before. The reflected ray was now brighter than the transmitted. To re-establish equality a disk with holes in a ring round the centre was rotated in the path. The ratio of the sum of the breadths of the holes to the whole circumference of the ring gave the percentage of the light that was reflected. For a piece of optically-worked blackened glass the amount reflected was ·058 of the total incident light. It was found that the amount of reflection depended greatly on the clearness and polish of the surface. Thus in one case re-polishing increased the amount from ·04095 to ·0445. Fresnel's formula gave in this case ·04514. Generally it appeared that the amount reflected was less than according to Fresnel's formula—a result contrary to that of Rood's. The numbers for polished glass and for silver on glass were ·94 and ·83.

ON THE NATURE OF SOLUTION[2]

IN connection with the discussion on the "Nature of Solution," in Section B, at the Birmingham meeting of the British Association, the following paper was read by Spencer Umfreville Pickering, Professor of Chemistry at Bedford College:—

The "hydrate" theory attributes dissolution to the existence, in a stable or partially dissociated condition, of definite liquid compounds (generally unknown in the solid form) of the substance dissolved and its solvent, and the mixing of these compounds with excess of the solvent.

In certain special instances we have direct evidence of the reality of such compounds,[3] but it is on general grounds rather than on any special experiments that I would seek to establish their existence.

There is, in the first place, a strong *primâ facie* improbability that substances such as copper sulphate, potassium hydrate, &c., which possess such an intense affinity for water, should be capable of existing in the anhydrous condition in the presence of an unlimited amount of water.

We know, moreover, that in a great number of cases—where a dehydrated salt is placed in water—hydration does undeniably precede dissolution,[1] and in such cases the salt can only exist in the liquid in the uncombined state if the continued action of the solvent is to decompose the hydrate which it has just formed. The only two forces by which such a decomposition might be supposed to be effected are (1) the attraction of the bulk of the water present for the few molecules of water combined with the salt; (2) the attraction of this same bulk of water for the (anhydrous) salt molecules. On the one hand, however, it is absurd to imagine that the mass of water molecules possess such a strong attraction for the few contained in the hydrate as to decompose this latter, or, even if they did, that they would ever have given them up to the salt in the first instance; and, on the other hand, it is equally absurd to urge the intensity of the attraction of the salt molecules for the water molecules as a reason for these two parting company.

Another general fact, which lends considerable support to the view that the dissolution of a salt is due to the formation of a hydrate, is, that those salts which *combine* with water always *dissolve* in that liquid, and, as a rule, the greater the energy with which they do combine with it, the greater is their solubility.

The thermal phenomena attending the act of dissolution point incontestably to the same conclusion. When a dehydrated salt (say $MgSO_4$) is dissolved in water a considerable evolution of heat occurs: and by the simplest experiment it can be established, beyond any possibility of doubt, that all, or the greater portion of this heat is due to the hydration of the salt. If the salt be taken in the hydrated condition less heat is evolved, and, without a single known exception, this evolution diminishes continuously as the salt taken is more and more highly hydrated; but even when taken in its most highly hydrated condition the evolution of heat is in many cases still very considerable.[2] Now, unless we can reconcile ourselves to attribute the heat evolution in this latter case to a cause entirely different from that which exists in the other cases,—unless we are content to shut our eyes to the proportionality between the heat evolved and the degree of hydration of the salt taken,—we must admit that even with a fully hydrated solid salt the heat evolved is due to further hydration; that not only do hydrates exist in solution, but that they are often of a higher order than the highest known in the solid condition.

Coming now to the other side of the question, we find many general considerations, as well as special results, brought forward against the hydrate theory of solution. The latter, however, are for the most part, I consider, urged on mistaken notions, and prove nothing *pro* or *con*.

Thus Dr. Nicol's study of the molecular volumes of salts in solution shows that their volumes are entirely uninfluenced by the presence or absence of water of crystallisation in the solid salt; that if any water is still combined with the salt when dissolved it acts in the same way, and is quite indistinguishable from the rest of the solvent present. In so far as his conclusion that these molecular volumes afford no evidence in support of the existence of combined water, I entirely agree with Dr. Nicol; but in concluding that therefore no water is combined, he has pushed his conclusions far beyond legitimate limits. The same reasoning that leads to the belief that the water and the salt bear no chemical relationship towards each other in solution would hold equally good with reference to the radicles of which the salts themselves are constituted, as Favre and Valson indicated in 1875 (*Comptes Rendus*, lxxv. 1000). Each radicle possesses its own specific volume entirely uninfluenced by the nature of the other radicle with which it is combined: the radicles behave independently, and as if there were no combination between them.

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[1] Abstract of a Paper read at the Birmingham meeting, 1886, of the British Association, by Lord Rayleigh.

[2] Continued from p. 22. From the *Chemical News*.

[3] See especially Berthelot, *Ann. Chim. Phys.* (5), 4, 445 to 537.

[1] Dr. Nicol (*Phil. Mag.* 1885, i. 453, and ii. 295) quotes experiments with sodium sulphate in opposition to this view. He shows that the dehydrated salt may dissolve in water under certain circumstances without any signs of previous hydration. When it does so, however, it forms a supersaturated solution, which is certainly very different from a normal solution, being, according to Dr. Nicol's determination of the solubility, due to the extension at lower temperatures of conditions which exist naturally only above 33°: but when it dissolves to form a *normal* solution it is with evident signs of hydration. Whatever this may prove as to the supersaturated solution, it certainly does not prove that the normal solution contains the anhydrous salt, —rather the opposite.

[2] Thus the "true" heat of dissolution of $MgSO_4 7H_2O$ is +7000 cal., and even this number is probably 1000 to 3000 cal. too low, as it contains no allowance for the heat of fusion of the $MgSO_4$ molecule. (See *Chem. Soc. Trans.* 1886, 279.)

Nor is it only from a study of the volumes of salts in the dissolved state that such results are obtained. Numerous determinations of the extent to which the vapour-pressure, the freezing-point, and the temperature of maximum density of water is influenced by the presence of various salts in it, have been made by Wüllner, Blagden, Dufour, Depretz, Rüdorff, and De Coppet,[1] with the general results that certain hydrates of the salt are in some cases present, and in others the salt is anhydrous; but these conclusions, which would tell more against the hydrate theory than for it, are eminently unsatisfactory. The whole question, however, has been re-opened by Raoult (*Ann. Chim. Phys.* (5), 28, 133; (6), 2, 66, 4, 401); and by an exhaustive extension of the work, and by including solvents other than water, and solids other than salts, he has thrown a new light on the subject. Not only does the salt, in its influence on the freezing-point, show no signs of the presence of combined water, but it shows no signs of itself being a single compound; each of the radicles contained in it acts independently of the other, and in precisely the same manner as a *molecule* of a non-saline substance (see especially *loc. cit.* 4, 426). Precisely similar conclusions as to the apparent non-combined state of radicles in a dissolved salt were arrived at by Valson in his work on capillarity (*ibid.* 1870), and by Hugo and Vrié (*ibid.* 1883) in their examination of the effect of membranes on salt-solutions. Other instances of a similar nature, physical and thermo-chemical, might be quoted.

That atoms or molecules which are undoubtedly united may retain their individuality so far as to act towards certain agents as if they were free, is surely not surprising; and from such methods as would lead us to conclude that the very radicles composing a salt are uncombined, it would be useless to look for evidence of the more feeble combination of the salt with its water, and inconsistent to argue, from the absence of such evidence, that no combined water is present.

Although I am not inclined to attribute any weight to these special experiments brought forward against the hydrate theory, it is otherwise with more general considerations.

The formation of hydrates cannot explain the absorption of heat which in many cases accompanies dissolution. The phenomena of solution are too universal to permit of imagining the existence of some definite compound of the dissolved substance with the solvent in every case. There is a continuous influence exerted by the salt on its solvent too extensive to be accounted for by the effect of mass on partially dissociated hydrates; there is a continuity between the fused and dissolved states in many cases, and a regularity in the variation of solubility with change of temperature, &c., which cannot be thoroughly explained on the hydrate theory.

However undeniable the existence of these compounds may be in many cases, they do not give an adequate explanation of all the facts of dissolution.

The hydrate theory can be neither rejected nor accepted.

The explanation of this contradiction is not, I think, very difficult to find. We are talking about molecules of solids and liquids, not as they exist, but as they do not exist. Our chemical formulæ for them represent but the results of analysis, or, at the most, the constitution of the substance in that transitory state of simplification which immediately precedes entire decomposition; what their composition may be when in the free state, and removed from all decomposing forces, we know not; all we do know or believe about them is, that they are then far more complex than chemical formulæ represent.

Crystalline form alone would show that a number, probably a very great number, of our so-called molecules combine together, bear certain definite relations and hold certain definite positions towards each other, producing a molecular aggregate or physical unit, which alone should receive the name of molecule.[2]

Just as a number of similar particles unite to form an aggregate or true molecule of any simple substance, so will dissimilar particles unite to form aggregates of a more complex nature.

It is but natural that our prejudices in favour of the "laws" of chemical combination and atomic valency, to which we owe so much, should lead us to attribute the variable composition of certain substances to our imperfect means of investigation rather than to the nonconformity of these substances to our laws. Whether we be right or not in our explanation, we must acknowledge that apparent *in*constancy in composition is one of the most marked features of immense classes of substances which cannot be termed other than chemical compounds.

The varied composition of minerals is said to receive an explanation in the statement that isomorphous substances may displace each other in definite proportions, but to an indefinite extent. This is undoubtedly true, but it does not obviate the necessity of recognising the existence of some form of attraction between these isomorphous substances. No purely mechanical or physical cause can explain this phenomenon; mere similarity of crystalline form has been proved to be incompetent to produce such results. A selective power is exhibited by the substances which thus unite,[1] as well defined as that selective power which in the case of simpler substances has received the name of chemical affinity, and the resulting compounds are characterised by the same uniformity in composition and physical properties[2] which is the attribute of acknowledged chemical compounds.

Nor is it with minerals and artificial crystals only that we find ourselves in what would appear to be a wide border-land between chemical compounds and mixtures. Whether we study the formation of alloys, the occlusion of gases by solids, ranging from the most mechanical action by insensible gradations to the formation of a substance having every appearance of a definite compound, or the decomposition of some of the firmest chemical bodies by so-called mechanical means (filtration), or the constant change in composition of many basic salts with change in the circumstances of their formation,—we are forced to admit that the definiteness which characterises the combination of atoms may be absent from, or at any rate unrecognisable in, the combination of our so-called molecules to form complex aggregates.

When we examine the constituents of these apparently indefinite compounds, it becomes clear that it is only substances which resemble each other which can combine in this manner; and one of the most striking features of dissolution offers such a strict parallel to this, that its meaning can scarcely be mistaken.

A certain degree of similarity in nature between the solvent and the substance dissolved is the invariable accompaniment of dissolution.

Dissolution, I believe, is but one of the many results of apparently indefinite chemical combination.

We cannot obtain a satisfactory explanation of the composition of minerals by admitting the existence of *definite* double salts only, nor can we explain the phenomena of dissolution by confining our attention to *definite* hydrates only. These may, and in all probability do, exist in solution, but they are only small circles within the larger ones; their successive formation and decomposition would give rise to irregularities and effects such as those which are observed in some cases; but these irregularities would form but ripples on the more regular changes which would accompany the variations in the molecular aggregates,—variations which, as in the case of minerals, would be so dependent on physical conditions as to obliterate their chemical nature when examined from many points of view.

The evolution of heat accompanying dissolution will still be attributable, as on the ordinary hydrate theory, to the formation of chemical compounds, but the far greater complexity, and, consequently, instability of these, than of atomic hydrates, if I may so call them, will remove all difficulty in comprehending the continuous effect of the mass of the solvent upon them, even when the latter exceeds that of the salt many hundred-fold; where heat is evolved, therefore, the evolution will be increased, though at a diminishing rate, by dilution.

The rapid increase in the heat of dissolution, produced by a rise of temperature, is but a necessary consequence of the formation of a chemical compound possessing a specific heat less than the sum of those of its components, and would of itself go far to prove that a solution did in reality contain such a compound. But a rise of temperature would also undoubtedly have another and opposing effect, for, being inimical to the complexity of these hydrates, they would be more dissociated at higher than at lower temperatures, and hence the heat of dissolution would not be so great as it should be according to the various specific heats. This is precisely what Dr. Tilden has proved to be the case (*Proc. Roy. Soc.* 1885, 401).

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[1] For a general summary and discussion of the results from the point of view of these physicists, see De Coppet, *Ann. Chim. Phys.* (4), 23, 366; 25, 502; and 26, 98.

[2] In a Paper read before this Section last year (Report, p. 989), I argued that our formulæ adequately represented the molecules of solids and liquids with which chemical reactions deal, although I fully recognised the existence of far more complex aggregates; my opinions have so far altered that at present I consider these aggregates to be recognisable in many operations which must be termed chemical, although in the great bulk of ordinary reactions the simpler or ultimate molecules need alone be considered.

[1] A power or "affinity" so strong that it will sometimes induce a salt to separate out in a crystalline form and with a proportion of water foreign to its nature, as well as from a solution too weak to yield it of its own accord (Aston and Pickering, "Multiple Sulphates," *Chem. Soc. Trans.*, 1886).

[2] J. M. Thomson, on the "Double Sulphates of Nickel and Cobalt" (*Brit. Assoc. Rep.* 1877, 209).

There is, however, another action which I believe accompanies every act of dissolution resulting in the *absorption* of heat.

The heat absorbed by a large number of salts in dissolving cannot be freely accounted for by the mere physical change of the solid into the liquid salt. Thus, the heat of dissolution of potassium nitrate is −8500 cal., and that of sodium nitrate −5000 cal., whereas the heat of fusion of these salts at the same temperature is but −1300 and −2300 cal. respectively. There must be some other heat-absorbing action besides the fusion of the salt. The amount of heat thus absorbed increases also with the dilution of the liquid. Moreover, we cannot, I think, account for the manner in which heat is evolved in one case and absorbed in another, or the way in which an absorption of heat sometimes gives place to an evolution, as the temperature or other conditions are changed, but by admitting the constant co-existence of two actions producing opposite thermal effects, and being influenced to different extents by an alteration of circumstances.[1]

On the theory which I am here advocating, this absorption of heat receives a ready explanation. Whatever be the complexity of the molecular aggregates of a liquid, those of a solid will be still more complex. Fusion would, therefore, entail their simplification; it would be but a chemical decomposition absorbing heat; this simplification would be pushed much further, however, when the salt is dissolved instead of being merely fused, for the particles of the liquid act chemically (*ex hyp.*) on those of the solid and combine with them themselves; the cold absorbed on dissolution would exceed that absorbed on fusion, and would, moreover, be increased by increasing the amount of the solvent. This accords fully with the facts observed.

All the phenomena attending dissolution are, therefore, I contend, accounted for by a full recognition of the real complexity of the units of matter, and by taking the more liberal view of chemical combination which is inculcated by a study of minerals and other substances. Every act of dissolution involves two actions. The chemical decomposition of the more complex aggregates of the solid into a simpler form, absorbing heat, and a chemical combination of these with the liquid, evolving heat; the only quantity which we can at present measure is the algebraic sum of these two.

Mr. Durham next gave a short statement of his own theory of solution:—

When, for example, common salt ($NaCl$) is placed in water, all the atoms act upon each other. The sodium of the salt acts upon the oxygen of the water, and the chlorine of the salt upon the hydrogen of the water; and the result is a definite compound, which we call a solution. The heat of formation of the acid is neutralised by the heat of formation of the oxide. If they be not equal, the difference is the heat of the solution; if they be equal, the heat is of course *nil*. If the former be the greater, the heat of the solution is negative; if the latter, it is positive. Solution arises from chemical affinity, and takes place inversely as the attraction between the positive element and the oxygen—and the negative element and the hydrogen—of water. But chemical affinity is itself physical; the atoms are physical, and all forces which act upon them must be physical forces. In a chemical mixture every atom is acting upon every other atom, but such action can be nothing else than physical; and we are therefore led to the conclusion that there is really no difference between chemical and physical action, and, consequently, that the alternative between the two does not exist.

In the course of the discussion, and preceding the reading of Mr. Pickering's paper, the following remarks were made:—

Dr. Armstrong said that, from the summary given by Prof. Tilden, it appeared that the two important questions for discussion were—(1) Does water of crystallisation exist in solution combined with the salt as it did prior to dissolution? and (2) What distinction is to be drawn between chemical combination and mechanical association or adhesion? In short, are the phenomena of dissolution of a chemical or of a mechanical character? But Prof. Tilden had made an important omission, inasmuch as he had not discussed the possible simplification of the molecules on dissolution: in discussing the evidence afforded by the various phenomena, everything turned upon the question whether the crystal molecules are of the composition represented by our ordinary formulæ, or are more or less complex.

As regards the first question, Prof. Tilden appeared to differ from Dr. Nicol, and to think that water of crystallisation did exist in solution. (Prof. Tilden, interposing, desired to explain that what he had said was that it was impossible, in the case of any solution, to say that one portion of the water is in combination with the salt and that another is not; all the phenomena of dissolution and dilution being continuous, no point can be found at which such a distinction can be set up. He believed that the salt was attached to all the water present without exception.)

Dr. Armstrong, resuming, said that much of the evidence appeared, he thought, to favour the conclusion that in certain cases water of crystallisation did exist in solution; *e.g.* the difference in colour between many hydrated and dehydrated salts taken in conjunction with the colour of their solutions. Again, many dehydrated salts dissolved much less readily than the corresponding hydrated salts: instances of this kind were not common among inorganic salts, but were often met with among organic salts, and the speaker cited calcium butyrate and certain naphthalene- and napthol-sulphonates as examples. Dextrose, again, ordinarily crystallises with two molecules of water, but if dehydrated and carefully dissolved in water at a low temperature it may be crystallised out from the solution in the anhydrous state. T. Thomsen's recent experiments, however, appeared to show that when two substances were dissolved in water they appropriated the water in the proportions in which they were present, thus favouring a purely mechanical interpretation of the phenomena of dissolution: but, on the other hand, it was to be noted that in the case of citric and sulphuric acids, for example, Thomsen's results were in accord with this conclusion only when it was assumed that the citric acid was present as the dihydrate, and sulphuric acid as the monohydrate, $H_2SO_4.OH_2$. In fine, the speaker was of opinion that while the question could not be regarded as settled, yet there was a considerable amount of evidence that the water was not evenly distributed, but was, in some cases at least, in part directly combined with the dissolved substance. Dr. Nicol had deduced an ingenious argument from J. Thomsen's observations on heats of neutralisation. As a criticism of Dr. Nicol's argument from the existence of neutralisation constants he would venture to say "Put not your faith in constants." If the views which he held—views which probably were at present peculiar to himself—were correct, the quantities in question ought to have a constant value. According to Helmholtz, all atoms hold a positive or negative electrical charge, a single charge being associated with a monad, two with a dyad, and so on. If when combination takes place these charges exactly neutralised each other, all compounds would be neutral and saturated; but actually this is not the case: in point of fact, there is no such thing as a saturated compound. Helmholtz seems to think that the charges may be held by different atoms with different degrees of force, but the speaker took a somewhat different view, and thought that probably when two atoms combined, in consequence perhaps of peculiarities of structure, their charges were not completely used up; the resulting molecules therefore possessed a certain residual charge or affinity, and were consequently in a position to enter into combination with other molecules. Thus water, he thought, was not a saturated compound; its oxygen atom was still possessed of residual affinity. The same was true of sulphuric acid. Consequently the two could combine together to form a hydrate. On neutralising a dilute solution of alkali by a dilute solution of acid, a stable condition is finally attained, and it is to be assumed that the affinities are fully satisfied, or very nearly so—that the charges practically neutralise each other: hence it may be expected that the heat of neutralisation will have nearly a constant value provided there be no disturbance such as the separation of a precipitate would produce. But the value of each of the several processes which go to make up the heat of neutralisation are entirely unknown to us, and in the absence of such knowledge it is impossible to place much confidence in arguments based upon the study of such complex phenomena.

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[1] A study of the thermal results attending the dilution of salt-solutions, established by Thomsen ("Thermo-chem.," iii., especially plate iv., and also the curves given by formic and acetic acids and by potassium and sodium hydrates), impresses very forcibly the co-existence of these two actions, although Thomsen himself does not seem to have noticed it.

As regards the question of chemical *versus* mechanical action, the speaker could only imagine one form of mechanical action attending dissolution, viz. that of the water molecules bombarding the surfaces of the solid, and as it were chipping off particles. All other actions, in so far as they could be regarded as involving the attraction of the molecules of the dissolved substance by those of the solvent, he was inclined to class as chemical. Nothing was more certain than that dissolution depended on the nature both of the solvent and of the substance dissolved. Like dissolves like—water is *the* solvent for bodies containing oxygen; sulphur compounds are dissolved by carbon bisulphide; phosphorus compounds by chloride of phosphorus; shale spirit, which is rich in olefines, and especially rosin spirit, which is rich in acetylenes and benzenes, were far better solvents of hydrocarbons and resinous bodies than petroleum, which consisted of saturated inert hydrocarbons, and was the worst of solvents. Facts such as these spoke strongly in favour of the conclusion that the phenomena of dissolution are largely of a chemical character.

Prof. W. N. Hartley was understood to base the argument in favour of the hydration theory chiefly on the changes of colour observed in the solution of certain salts in various proportions of water. The chlorides, bromides, and iodides of cobalt, nickel, and copper exhibit these phenomena most plainly. Thus the iodide of cobalt in the anhydrous state is black, its dihydrate is green, the hexhydrate a reddish brown. If this last be dissolved in water a pink solution is formed, which probably contains a richer hydrate. The brown saturated solution of the hexhydrate is a very dense liquid, of specific gravity about 3, and when water is added to it the formation of the pink liquid is attended by a large evolution of heat, and this affords evidence that the hydrate exists in the solution. Again, hydrated cupric chloride contains two molecules of water, and when quite dry is of a pale blue colour. Its solution in water has the same colour unless it be heated, and then it turns green. Nickel salts behave similarly. So that the evidence, on the whole, warrants the belief that when a hydrated salt is dissolved in water the water of crystallisation remains a constituent part of the molecule.

Dr. Gladstone commenced his remarks by a discussion of the question, What is a salt in solution? Is the solution of a salt in water a process analogous in any degree to the decomposition which takes place when one salt is mixed with another? Take, for instance, chloride of sodium and water. Many years ago the speaker had endeavoured to determine whether any chemical decomposition of the salt by the water occurred so as to give rise to sodium hydrate and hydrochloric acid, but he had come to the conclusion that this decomposition took place, if at all, only to a very small extent. Many salts, as had already been stated, combine with water to form coloured hydrates, and the hydrate is of a colour different from that of anhydrous salt. But a coloured hydrate, when dissolved in a sufficient quantity of water, is never changed by further dilution. The speaker had endeavoured to ascertain whether the specific refraction of substances was altered by solution. He had found that no alteration could be detected, and this result was afterwards confirmed by the experiments of other chemists. The refraction equivalent of a solution is equal to the sum of the refraction equivalents of the salt and the water present. In an alum solution, the water of crystallisation supposed to be in combination with the salt is not distinguishable by its refractive power from the water of solution outside it. It seems impossible, however, to arrive at a conclusion with regard to the constituents of a solution. The idea of reciprocal decomposition is not supported by experimental evidence, save in some exceptional cases, and the actual condition of a dissolved salt seems beyond expression by formulæ.

TEN YEARS' PROGRESS IN ASTRONOMY[1]

THE Earth.—In what may be called the astronomy of the earth there is no very great discovery, nothing extremely new and brilliant to record during the past decade; but there has been considerable and steady progress.

(*a*) As regards the earth's form and dimensions, it has become quite certain that Bessel's ellipticity (1/300) is too small. Clarke's value of 1/294 is now admitted and employed on the U.S. Coast Survey with a decided improvement of accordance. A slightly larger value even is suggested by the most recent pendulum observations, and 1/292 is now adopted in Europe.

One of the most important steps in this branch of investigation is the discovery by Mr. Peirce (of our own Coast Survey), of the large correction required in many former pendulum determinations, on account of the yielding of the stand from which the pendulum is suspended.

During the past ten or fifteen years a great amount of material has been collected towards a complete gravitational survey of the earth, by the work of Lieut.-Col. Herschel in India, and of the officers of the Coast Survey in this country and elsewhere, and a very important part of it has consisted in connecting the older work with the new, by Peirce's operations in Europe, and those of Herschel in this country.

At the same time it has become increasingly evident that very little is now to be gained by endeavouring to find a spheroid fitting the earth's actual form more closely. It will be best simply to adopt some standard (say that of Clarke, but it makes very little difference what), and to investigate hereafter the local deviations from it. These deviations seem to be larger and more extensive than used to be supposed, the station errors in latitude and longitude being at least quantities of the same order as the variations of elevation.

We mention, in passing, the investigations of Fergola, based on observations at Pulkowa and Greenwich, and leading to a suspicion that the axis of the earth is slightly changing its position and shifting the place of the Poles on the earth's surface. Operations have been organised to determine the question by co-operation between different observatories in nearly the same latitude, but widely differing in longitude.

Nor ought we to pass unnoticed an elaborate paper by Kapteyn, of Groningen, on the determination of latitude by a method depending upon time-observation of stars, at equal altitudes, though in widely different parts of the sky; the stars being so selected that all errors of star-places, instrument, and clock, are almost perfectly eliminated. In the same connection we ought to mention also the new equal-altitude instrument, the Almucantar, invented by Chandler, of Cambridge, and his development of the method of determining time by its use. It may possibly supersede the transit instrument for this purpose, as he seems to expect, though we think it hardly likely.

Rapid progress has been made in determining the difference of longitude between all the principal parts of the earth. There now remain very few stations of much importance which have not their longitude from Greenwich telegraphically settled within a small fraction of a second. In Europe Albrecht has combined into a consistent whole all the different data for more than one hundred points. Our American system has been similarly worked out by Schott, and is connected with the European by no less than four different and independent cable-determinations. South America is connected with the United States by the recent operations of our naval officers in the West Indies and along the eastern and western coasts of the continent; and with Europe by a cable connection between Lisbon and Pernambuco, also effected by them. It is worth noting that two large errors in European longitudes owe their detection to American astronomers. The difference of longitude between Greenwich and Paris was corrected by our Coast Survey in 1872 to the extent of nearly half a second of time, and our naval officers in 1878 showed that the then received longitude of Lisbon was 8·54s. too small! It is a less surprising fact that an error of 35s. was found in the longitude of Rio.

Our navy has also determined an important series of telegraphic longitudes along the eastern coast of Asia and through the East Indies. The French have been doing similar work in the same regions, especially in connection with the transits of Venus; and the English have determined a large number of longitudes in India. These Asiatic longitudes have been recently connected with Australia and New Zealand by English astronomers, and a telegraphic longitude connection has been effected down the eastern coast of Africa from Aden to the Cape; so that now it is perfectly practicable, if it is desirable, to have one standard of time in all the civilised world.

A word perhaps is here in place as to this question of standard time and the beginning of the day. The adoption by our railroads of the system of standards differing from Greenwich time only by entire hours has, I think, been admittedly a great step in advance, as regards public convenience and safety in travelling. At a few points, where standard and local time happen to differ by nearly the maximum possible amount of half an hour, some annoyance is felt, and there is still some opposition; but it seems quite clear that, in this country at least, all resistance will soon die out.

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[1] "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young. Read May 17, 1886, before the New York Academy of Sciences.

As regards the more purely astronomical question of making the astronomical day coincide with the civil day, by beginning at midnight, instead of noon, as it does at present, there is more difference of opinion. For my own part, I am frankly in favour of the change, because I see no use in perpetuating an anomaly which is sometimes annoying and confusing. At the same time the change would, of course, involve some inconvenience to computers and night-observers, and it must be admitted that at present a large number, and possibly a majority, of the most eminent astronomers, in other countries as well as in this, are opposed to it. Those of us whose work falls about as much in the day as in the night, and those, I think, who take a long look ahead, are in favour of the reform; but those whose work is mainly nocturnal, or is based on observations made chiefly at or near the "witching hour," dread the inconvenience of a change of date in the midst of the record, and the risk of confusion in the interpretation of old observations.

The question, however, seems to me not a very important one. I notice that the visitors of the Royal Observatory have just recommended that the change be introduced into the British *Nautical Almanac* for 1891.

Before passing to the moon, a word should be added as to the outcome of the most recent investigations regarding the steadiness of the earth's rotation. Some irregularities in the lunar motions have appeared to justify a suspicion, at least, that they might be caused by irregularities in the length of the day. The researches of Newcomb upon ancient eclipses and occultations of stars give results not necessarily inconsistent with this hypothesis, perhaps even slightly in its favour, but his careful examination of the past transits of Mercury contra-indicates it pretty decidedly.

The Moon.—During the past ten years there has been no work upon the lunar theory quite on a level with that of Hansen, Delaunay, Plantamour, and Adams in the years preceding; but the labours of Neison, Hill, and Newcomb well deserve mention. The former especially has carried his approximations to a considerably higher point than any of his predecessors, though not without making a few numerical mistakes, which have been detected and corrected by Hill. The investigation of ancient and mediæval observations of the moon by Newcomb is also a very important work, as showing clearly that the lunar theory is still incomplete, and that it is impossible by any tables yet made to represent accurately the whole series of observations. A value of the secular acceleration which suits the observations of the last 200 years will not fit the Arabian observations made 1000 years ago, nor will it satisfy the eclipse observations of still more ancient date, unless at least the received interpretation of those ancient eclipses be admitted to be wrong, as Prof. Newcomb seems to consider rather probable. From his discussion he derives for the secular acceleration a value of 8″·4, as against the value of 12″·1, deduced by Hansen.

It will be remembered probably by every one present that the *theoretical* value of this quantity is about 6″, and that Ferrel, Adams, Delaunay, and others, attributed its apparent increase to 12″ to the action of the tides in retarding the earth's rotation and so lengthening the day; if Newcomb's value is correct, this tidal retardation is cut down from 6″ to about 2″·5.

The study of the moon's surface has been carried on with assiduity, but I do not know that any remarkable results have been reached, though Klein's observation, in 1877, of what he supposed to be a newly-formed crater (Hyginus N.), excited a good deal of interest and discussion for a number of years; and the most eminent selenographers are still divided in opinion on the question.

The publication by the German Government of Schmidt's great map of the moon, in 1878, unquestionably marks an epoch in selenography; and the photographic work of Pritchard, and the heliometric determination of the moon's physical libration by Hartwig, must not pass unnoticed.

Probably, however, the lunar work which has drawn to itself most attention and interest is the investigation of the moon's heat by Lord Rosse and Prof. Langley.

The earliest observations of the kind date back now forty years, when Melloni, in 1846, first detected the moon's heat by means of the then newly-invented thermopile. But the first really scientific *measurements* are only about fifteen years old, due to Lord Rosse, at Parsonstown, and to Marie Davy, at Paris; and they seemed to show that at the time of full moon we receive from our satellite, not merely *reflected* heat, but warmth *radiated* from the moon's surface; as if this surface were raised to a considerable temperature by the long insolation to which it has been exposed during the preceding fortnight. Lord Rosse estimated the probable temperature of this heated rock to be as high as from 300° to 500° F.

But within the past four or five years this conclusion has been called in question. Observations at Parsonstown, of the rapid diminution of radiation during a lunar eclipse, seem to favour the newer view that the moon's surface, like that of a lofty mountain-top on the earth, never gets very hot, since the absence of air enables the solar heat to escape nearly as fast as it is received.

Prof. Langley's recent and still progressing work upon this subject far excels in delicacy and elaborateness anything done before. At first it seemed to show that the temperature of freezing water was never reached even at the hottest parts of the lunar surface; but the later observations throw some doubt on the legitimacy of this inference. It is found that the radiation from the moon unquestionably contains a considerable percentage of rays which have a wave-length *longer* than any of the heat-rays from melting ice; and this fact has been supposed to make it probable that the moon's surface was colder than the ice. But then, within a few weeks, Prof. Langley has found the long-waved rays in the radiation from an *electric arc!* So the question still hangs debatable.

The Sun's Parallax—I think we may say that, during the past ten years, substantial progress has been made with the problem of the solar parallax. The transit of Venus in 1882 adds whatever value its results may have to those obtained eight years before; but, on the whole, so far as can be judged from the reductions thus far completed and published, it would seem likely that the outcome of the transit observations will be simply to confirm the results obtained by other methods. It may be that the data obtained from the German heliometer measurements will prove more accordant and decisive than those derived from photographs and from the contact observations; there are flying rumours that they will, but it will be necessary to await the official publication for certain knowledge on this point. If they do not, we shall be obliged, hereafter, to relegate transit observations to a secondary rank, as a means of determining the sun's distance. From the various observations of the two transits, different computers have deduced values of the parallax all the way from 8″·6 to 8″·95, corresponding to a distance ranging from 95,000,000 to 91,500,000 miles.

The case is quite different with the heliometer observations of the opposition of Mars, in 1877, made by Mr. Gill at Ascension Island. These give, in a most definite and apparently authoritative manner, a value of 8″·783, and are apparently irreconcilable with any value much greater than 8″·81, or less than 8″·75. So far as can be judged from the number, nature, and accordance of the observations, I believe we must accept this as the most trustworthy of the geometrical methods yet employed; though the weight of the result would certainly be increased if it did not depend to such an extent upon the work of a single individual.

The confidence of astronomers in the correctness of this value is greatly fortified by the fact that the most recent and reliable determinations of the velocity of light, made by Michelson and Newcomb, in 1877, 1880, 1881, and 1882, when combined with the Pulkowa constant of aberration determined by Nyrèn from all the data available up to 1882, give a solar parallax accordant with the preceding almost to the hundredth of a second—8″·794 as against 8″·783. It is true there are possible theoretical objections to the method; as, for instance, that the result may be slighted affected by the motion of the solar system through space. Enough is not known certainly about the constitution of the medium that transmits light through space, to decide all such questions *a priori* and authoritatively; but it is unquestionable that any correction needed on account of such possible causes of error must be very minute.

We believe, therefore, that it is safe to assume pretty confidently that the solar parallax is about 8″·8 (though probably a trifle less), which makes the sun's mean distance 93,000,000 miles, with an error not likely much to exceed 150,000 miles. A larger value of the parallax (about 8″·85) still holds its ground in the nautical almanacs, and undeniably is nearer the *average* of the results given by *all* the known methods. But none of the other methods seem to us to compete at all in precision with the two whose authority we accept.

The Sun and Meteorology.—The study of the solar surface has been carried on very persistently by Spörer, in Germany, as well as by others, and a great amount of material has been collected bearing upon the theory and nature of sunspots, and their periodicity. The extensive series of photographs obtained at Kew, and at Dehra Doon, in India, constitutes almost a continuous record of the solar surface for several years. The relation between this periodicity and terrestrial conditions has been assiduously examined, but on the whole the outcome seems to me to leave this connection as doubtful as it ever was, in most cases at least. While in some parts of the earth it looks as if there were a slight but marked increase of storm and rainfall at the time of sunspot maximum, the reverse seems to be true in other countries. In South America, Dr. Gould thinks that he has demonstrated a very perceptible effect of the condition of the sun's surface in modifying the strength and direction of the winds; but thus far similar investigations elsewhere show no such result. It will evidently be necessary to wait for a longer and more widely extended collection of statistics to settle the question. We do not even know as yet whether we get more or less than the average heat from the sun during the sunspot maximum.

But I think it may be set down as certain that the condition of the sun's surface exerts, if perhaps a real, yet only a very slight effect upon our earthly meteorology. With terrestrial magnetism the case is markedly and singularly different, and one of the most interesting problems now pressing for solution is the nature of the connection between solar disturbances and magnetic storms.

Solar Heat.—A great deal of labour has been expended upon the study of the sun's heat during the last decade. The investigations that strike me on the whole as most worthy of mention are those of our own Langley and of the Italian Rosetti, whose early death a few months ago is a great loss to science. Secchi and Ericsson, on the one side, had contended for a solar temperature of some millions of degrees, basing their results on Newton's law of cooling; while, on the other, Crova and Violle, from their measures of the solar radiation, reduced according to the so-called law of Dulong and Petit, maintained that the temperature does not much exceed that of many terrestrial furnaces, somewhere from 1500° to 2500° C. Rosetti's experiments upon the radiation of the electric arc and other sources of intense heat showed pretty clearly the inapplicability of Dulong and Petit's law to high temperatures, and indicate a solar temperature not far from 10,000° C., or 18,000° F. But they also make it clear that the limits of uncertainty are still very great.

Prof. Langley, by his invention of the bolometer, has been able to investigate separately the amount of energy transmitted to the earth in the solar rays of every possible wave-length, and to determine the effect of our atmosphere in absorbing each kind of ray. He has shown that the older method of investigating this solar radiation, *in a lump* so to speak, gives fallacious results on account of atmospheric absorption; and that the necessary correction compels us to increase our estimate of the sun's energy at least 20 per cent. In my own little book upon the sun, published in 1881, I had set the so-called solar constant at twenty-five calories per square metre per minute. It is now certain that it must be put at least as high as thirty. Prof. Langley's investigations seem also to show another remarkable fact—that we do not receive from the sun any at all of the low-pitched, slowly-pulsing waves, such as we get from surfaces at or below the temperature of boiling water. The solar spectrum appears to be cut off abruptly at the lower end; and this cutting off we know cannot have been effected in the earth's atmosphere, because we receive from the moon in considerable quantity just this very sort of low-pitched rays. Langley finds them also abundant in the radiation of the electric arc, so that we can hardly suppose them to be *originally* wanting in the solar heat. It now looks as if we must admit that they have been suppressed either in the atmosphere of the sun itself, or in interplanetary space. Another striking conclusion first clearly pointed out by Langley is that, if the sun's atmosphere were removed, its light would be strongly blue.

The Solar Surface and Spots.—As regards the general make-up of the solar surface, I do not think there has been any new fact of extreme importance brought out within ten years. Janssen has, however, carried solar photography to higher excellence than ever attained before, and has obtained plates that show the "granules" and their grouping on a scale previously unknown. He thinks that his plates prove a peculiar constitution of the solar surface, consisting in collections of clearly-defined and rounded granules, separated by regions or streaks where they are ill defined and elongated; and he calls the phenomenon the "reseau photosphérique," or photospheric network. According to him the "net" remains approximately constant for some minutes at a time, as shown by plates taken in quick succession, but is subject to rapid and enormous changes in periods exceeding a quarter of an hour or so. I find some scepticism among high authorities as to the trustworthiness of his conclusions. There are suggestions that the appearances presented may be due to currents of air in the telescope tube and at the surface of the sensitive plate; but I am disposed to think he is right, for, on several occasions when the seeing has been exceptionally fine, I have observed with my own eyes something quite analogous, in our large telescope at Princeton.

The spots have been carefully studied by several observers, by Spörer especially, in a statistical way, and by Vogel, Lohse, Tacchini, and others, as to structure and detail. Spörer has brought out very clearly the connection between the number and average latitude of the spots. It appears that, speaking broadly, the disturbance which produces the sunspots begins in two belts on each side of the sun's equator in a latitude of over 30°; these belts or spot-zones then gradually move in towards the equator, the sunspot maximum occurring when their latitude is about 16°, while the disturbance gradually and finally disappears at a latitude of 8° or 10°, some twelve or fourteen years after its first appearance. But two or three years before this disappearance a new zone of disturbance shows itself in the same latitude as its predecessor, so that for a while, about the time of sunspot minimum, there are two well-marked zones of spots on each side of the sun's equator—one pair near the equator, due to the expiring disturbance which began some ten or eleven years ago; the other far from the equator, and due to the newly-arising outburst, which will reach its maximum in three or four years, and then pass away like the former.

There can be no doubt that the phenomenon is a very significant one, but its explanation, like that of the periodicity itself, is still to be found.

Nor is the problem of the spots themselves yet fully solved. Not that there is any reasonable question that they are *hollows* in the solar photosphere; but how they originate, how deep they are, and what are the causes of their darkness, and the condition and temperature of the darkening substance—these are questions to which only uncertain answers can now be given. A long and important series of observations upon the widening of the lines of certain elements in the sunspot spectra has been made by Mr. Lockyer, and establishes clearly the fact that those lines, of *iron* for instance, which are conspicuously black and wide in the sunspots, are often just those which do *not* show themselves conspicuously in the prominences; and moreover both in spots and prominences the iron lines that do show themselves are most frequently those which closely coincide with lines in the spectra of other substances. Singularly, also, and so far quite without explanation, it appears according to his observations that at the sunspot maximum those *iron* lines which at other times are conspicuous in spot-spectra entirely disappear.

Perhaps I may be allowed to mention here a recent observation of my own upon these spot-spectra: with a high dispersion the darkest part of the spot-spectrum is found to be not continuous, but made up of fine lines overlapping or almost touching each other, with here and there a clear space left, like a fine bright line. It means, I think, that the absorbing vapours which darken the interior of the spot are wholly gaseous, and tends to disprove the idea that they are mostly of the nature of smoke or steam. We mention also, in passing, another thing which has been shown by our large instrument at Princeton—that the apparently bulbous, finger-tip-like terminations of the penumbral filaments are often, under the best circumstances of vision, resolved into fine, bright, sharp-pointed hooks which look like the tips of curling flames.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the biennial election of members of the Council of the Senate, Prof. Michael Foster and Dr. Donald MacAlister were elected to serve for four years.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, November 11.—Mr. J. W. L. Glaisher, F.R.S., President, in the chair.—Mr. F. S. McAulay, St. Paul's School, was elected a Member.—The following gentlemen were elected to form the Council for the ensuing session:—President: Sir J. Cockle, F.R.S.; Vice-Presidents: J. W. L. Glaisher, F.R.S., Prof. Harry Hart, and the Right Hon. Lord Rayleigh, Sec.R.S.; Treasurer: A. B. Kempe, F.R.S.; Hon. Secretaries: Messrs. M. Jenkins and R. Tucker; other Members: Prof. Cayley, F.R.S.; E. B. Elliott, Prof. Greenhill, J. Hammond, Prof. M. J. M. Hill, C. Leudersdorf, Capt. Macmahon, R.A., S. Roberts, F.R.S., and J. J. Walker, F.R.S.—The retiring President, J. W. L. Glaisher, F.R.S., delivered an address, which treated of the Mathematical Tripos Examinations at Cambridge, and of the bearings of recent changes in the same upon the advancement of mathematics.—The following communications were made:—Certain operators in connection with symmetric functions, by R. Lachlan.—The transformation of a certain quartic elliptic element, by R. Russell.—Discussion of a multilinear operator, with applications to the theories of invariants and reciprocants, by Capt. Macmahon.—The theory of screws in elliptic space (fourth note), by A. Buchheim.—The rectification of certain curves, by R. A. Roberts.—Rectification of a sphero-conic, by H. F. Burstall.—Third paper on reciprocants, by L. J. Rogers.—The "sine-triple-angle" circle, by R. Tucker.

Linnean Society, November 4.—Mr. William Carruthers, F.R.S., President, in the chair.—The President paid a passing tribute to, and commented on the loss sustained in the death of, Mr. George Busk, a former Secretary and Vice-President of the Society. Afterwards he drew attention to phosphorescent organisms obtained by him in the Firth of Clyde last September.—Mr. John Murray also made remarks on the same, alluding to his own observations of *Ceratium tripos* being found in long chains in the ocean ("Narrative of the Cruise of the *Challenger*"), and to Klebs's opinion of *Ceratium* being a genus of unicellular Algæ, and not an infusorian.—Prof. J. Macoun made remarks on a series of cones of Canadian *Piceas*. He showed that the various forms occurring from east to west of the continent, which had been hitherto considered different species, were doubtless local varieties of but one species, slightly modified according to the altitudes and region they inhabited.—Dr. F. Day exhibited a salmon parr, twenty months old, raised at Howietoun from parents which had never visited the sea. He also showed coloured drawings of hybrids raised in the same establishment—one being a cross between the American charr and the Loch Leven trout, a second between the American and the British charr, and a third between the last-mentioned hybrid and the Loch Leven trout; all were fertile.—Fresh specimens of a white variety of *Crocus nudiflorus* from Biarritz, France, were shown for Mr. W. D'Arcy Godolphin Osborne, who first observed the variety there in 1882, but since then it has been figured by Mr. G. Maw in his monograph of the genus.—Mr. E. M. Holmes exhibited examples of *Lycoperdon echinatum*, Pers., viz. the young plants, and the reticulate appearance of the peridium left by the falling off of the spines.—Mr. F. Pascoe exhibited one of the round olive green balls from Sicily, formed by the action of the sea on fragments of the *Posodonia caulinia*, and reduced, after a few days' exposure, to a flat cake-like body densely covered with minute crystals of salt. He also showed some acorn-shells (*Balanus*), where several individual animals had united their shells into a common tube, and where the outer valves of each animal had lengthened, forming a series of irregular subsidiary tubes radiating from the apex of the primary one.—Mr. E. C. Bousfield read a paper on the natural history of the genus *Dero*. In this he gives a full account of their habits and the best method of observing them. The *Naias digitata*, Müll., he rejects as a specific appellation, Müller's reference being defective. He shows wherein the Deros differ from the Naiades, viz. in the former having a respiratory apparatus at the end of the tail. He diagnoses seven species, four being new; all are figured.—Mr. S. O. Ridley gave in abstract his researches on the genus *Lophopus*, and description of a new species from Australia. This latter, *L. lendenfeldi*, obtained by Dr. Lendenfeld near Sydney, N.S.W., is distinguished from *L. crystallinus* by length of tentacles equalling the body of the polypide, and by the non-pointed outline of the statoblast. The new species is the fourth fresh-water Polyzoon recorded from Australia, and the first of its genus satisfactorily determined from the southern hemisphere. Staining with borax-carmine brings out multipolar cells in the ectocyst, indicating mesodermal elements, and denoting that the ectocyst is something more than mere epithelium. A modification of the diagnosis of the genus is necessitated from these observations.

Chemical Society, November 4.—W. Crookes, F.R.S., Vice-President, in the chair.—Messrs. H. Crompton, G. Dyson, T. B. Tyson, and S. Williamson were admitted Fellows of the Society.—The following papers were read:—The action of chlorosulphonic acid on naphthalene-α- and β-sulphonic acids, by Henry E. Armstrong and W. P. Wynne, B.Sc.—The action of bromine on (Schäfer's) β-naphtholsulphonic acid, by Henry E. Armstrong and F. W. Streatfeild.—The action of bromine on the naphthalenesulphonic acids, by Henry E. Armstrong and W. P. Wynne, B.Sc.—α-Nitro-, α-bromo-, and α-chloronaphthalenesulphonic acids, by Henry E. Armstrong and S. Williamson.—The hydrolysis of sulphonic acids, by A. K. Miller, Ph.D.—The action of bromine on tolueneparasulphonic acid, by A. K. Miller, Ph.D.—Phosphorus tetroxide, by T. E. Thorpe, F.R.S., and A. E. Tutton.—Conversion of ditolane-azotide into diphenanthrylene-azotide, by Francis R. Japp, F.R.S., and Cosmo Innes Burton, B.Sc.—A chemical study of vegetable albinism; part 3, experiments with *Quercus rubra*, by A. H. Church.—The synthetical formation of closed carbon-chains; part 2, some derivatives of tetramethylene, by W. H. Perkin, Jun., Ph.D.—The action of the halogens on the salts of organic bases; part 2, tetramethylammonium salts, by Leonard Dobbin, Ph.D., and Orme Masson, M.A., D.Sc.—Glycyphyllin, the sweet principle of *Smilax glycyphylla*, by Edward H. Rennie, M.A., D.Sc.

Entomological Society, November 3.—Mr. Robert McLachlan, F.R.S., President, in the chair.—The following gentlemen were elected Fellows:—Messrs. P. Cameron, F. Archer, H. J. S. Pryer, H. Norris, N. P. Fenwick, J. Brown, J. P. Tutt, and A. P. Green.—Mr. E. B. Poulton exhibited a mass of minute crystals of formate of lead, caused by the action of the secretion of the larva of *Dicranura vinula* upon suboxide of lead. He stated that a single drop of the secretion had produced the crystals which were exhibited; and he called attention to the excessively high percentage of formic acid which must be present in the secretion.—Mr. S. Stevens exhibited a specimen of *Laphygma exigua*, recently captured by Mr. Rogers in the Isle of Wight.—Mr. W. F. Kirby exhibited, and read notes on, a specimen of *Perilampus maurus* recently bred by Mr. Walter de Rothschild from *Antheræa tirrhea*, one of the rarer South African Saturnidæ.—Mr. T. W. Hall exhibited a number of specimens of *Xanthia fulvago* (*cerago*), somewhat remarkable in their variation, and showing a graduated series, extending from the pale variety, *flavescens* of Esper, to an almost melanic form.—Mr. Boyd exhibited, and made remarks on, the larva of a species of *Ornithoptera* from New Guinea.—Mr. H. Goss exhibited a series of *Bankia argentula* collected by him in Cambridgeshire in June last; and also, for comparison, a series of specimens of the same species taken at Killarney in June 1877. It appeared that the Irish form of the species was larger and more brightly coloured than the English form.—Mr. Eland Shaw exhibited a female specimen of *Decticus verrucivorus* taken in July last at St. Margaret's Bay, Kent.—Mr. Waterhouse recorded the recent capture of *Deiopeia pulchella* at Ramsgate, by Mr. Buckmaster; and the capture of *Anosia plexippus* at Gibraltar was also announced.—Mr. J. W. Slater read a paper on the relations of insects to flowers, in which he stated that many flowers which gave off agreeable odours appeared not so attractive to insects as some other less fragrant species; and he stated that Petunias, according to his observations, were comparatively neglected by bees, butterflies, and Diptera. Mr. Distant, Mr. Stainton, Mr. Weir, Mr. Stevens, and the President took part in the discussion which ensued, and stated that, in their experience, Petunias were often most attractive to insects. Mr. Stainton referred to the capture, by himself, of sixteen specimens of *Sphinx convolvuli* at the flowers of Petunias, in one evening in 1846.—Jonkeer May, the Dutch Consul-General, asked whether the reported occurrence of the Hessian fly (*Cecidomyia destructor*) in England had been confirmed. In reply, Mr. McLachlan stated he believed that several examples of an insect, thought to be the Hessian fly, had been bred in this country, but that everything depended upon correct specific determination in such an obscure and difficult genus as *Cecidomyia*.

EDINBURGH

Mathematical Society, November 12.—Dr. R. M. Ferguson, President, in the chair.—The President gave a retiring address, for which, and for the gratuitous use of the rooms of the Edinburgh Institution for the Society's meetings a vote of thanks was awarded to him.—Mr. J. S. Mackay read a paper on the solutions of Euclid's problems with a ruler and one fixed aperture of the compasses by the Italian geometers of the sixteenth century; and communicated a note from Mr. R. Tucker giving some novel properties connected with the triangle.—Mr. A. Y. Fraser read a note by Mr. William Renton on the equivocal sign.—The following office-bearers were elected for the session:—President: Mr. George Thom; Vice-President: Mr. W. J. Macdonald; Secretary: Mr. A. Y. Fraser; Treasurer: Mr. John Alison; Committee: Mr. R. E. Allardice, Dr. R. M. Ferguson, Mr. George A. Gibson, Mr. William Harvey, Mr. J. S. Mackay, Mr. Thomas Muir.

SYDNEY

Royal Society of New South Wales, September 1.—Mr. Ch. Rolleston, President, in the chair.—Mr. Fredk. B. Gipps, C.E., read a paper on "Our Lakes and their Uses." The lakes of New South Wales being all liable to dry up, Mr. Gipps stated that it is possible, however, to impound large artificial lakes. The leading features of Lake George were described, and a means of utilising its waters for irrigating a large area were entered into.—A very beautiful specimen of gold from calcite was exhibited by Dr. Leibius, of the Mint. The lime having been dissolved in acid, the gold was left as a network of the finest ramifying filaments.

PARIS

Academy of Sciences, November 8.—M. Jurien de la Gravière, President, in the chair.—On the relations of geodesy and geology, by M. Faye. This is a reply to M. de Lapparent's recent criticisms of the author's well-known views on the relations of the geodetic and geological sciences. M. de Lapparent's objections are treated in detail, and it is argued that the law of unequal cooling of the terrestrial crust dates back to times anterior to the astronomico-geological epoch, when the seasons began to be established. It controlled the whole series of geological evolutions from the first formation of the oceanic basins.—Thermic researches on the reactions between ammonia and the magnesian salts, by M. Berthelot. These studies tend to define the action of ammonia on the magnesian salts, determining the analytical conditions which enable magnesia to be separated from the other alkaline salts, and showing that, by union with sulphuric acid or with hydrochloric acid, the complex ammoniaco-magnesian base liberates a quantity of heat greater than pure ammonia or pure magnesia, and very near that liberated by potassa and soda.—On the incandescent substance in fusion recently reported to have fallen during a thunderstorm at Luchon, by M. A. Trécul. In connection with M. St. Meunier's remarks on this subject, the author refers to a communication made by him to the Academy in 1881 (*Comptes rendus*, xcii. p. 775), showing that in thunder-clouds there may exist an incandescent substance in fusion, which under certain conditions may fall to the ground in the form of variable drops or globules.—Report made on behalf of the Section for Chemistry on M. Moissan's researches relating to the isolation of fluor, by M. Debray. After describing the attempts made by Davy and subsequent chemists to solve this problem, the report gives a detailed account of M. Moissan's researches, and considers his final conclusion fully justified, that the gas liberated by the electrolysis from the pure anhydrous hydrofluoric acid, with which he experimented, is undoubtedly fluorine. The question consequently now enters on a new phase, and chemistry may henceforth deal directly with fluorine, and attack problems of great interest, formerly regarded as insoluble.—Observations of the new planet 261, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—On an extended class of uniform transcendants, by M. H. Poincaré.—On Maclaurin's series in the case of a real variable: application to the development of a homogeneous body in series of the potential, by M. O. Callandreau.—Note on the octahedron, by M. P. Serret.—On the transport of force; a reply to M. Deprez, by M. Hippolyte Fontaine. It is admitted that the principle of a series of machines linked together is not new; but the author claims that the results of his researches obtained by the employment of special dynamos, and by a rearrangement of the mechanical elements, must be regarded as new. The transport of 50 horse-power through a resistance of 100 ohms with a loss of only 48 per cent., by employing dynamos jointly weighing only 8400 kilograms and costing only 658*l.*, is now realised for the first time.—Determination of the heats of neutralisation of the malonic, tartronic, and malic acids: remarks on the heats of neutralisation of the homologous acids of oxalic acid and of the corresponding hydroxyletted acids, by MM. H. Gal and E. Werner. The results here tabulated of the authors' researches show that the heat of neutralisation of the homologous bibasic acids under consideration diminishes in proportion as the molecular weight increases.—General methods of crystallisation by diffusion; reproduction of mineral species, by M. Ch. Er. Guignet. These experiments are described as a generalisation of M. Becquerel's ingenious researches on the slow action set up between two liquids separated by a film, a porous wall, or even a glass tube with a fissure or capillary aperture. The methods employed are applicable to a large number of bodies, and yield crystals in any required quantities.—Synthesis of conicine, by M. A. Ladenburg. The processes are described by which the author has succeeded in obtaining synthetically three bases possessing the same mutual relations as racemic and tartaric acids, one of which is absolutely identical with natural conicine.—On the chemical transformations brought about by the action of solar light, by M. E. Duclaux. Having already examined the sterilising action of solar light on microbes, the author here proceeds to show that the influence at play belongs to the order of chemical phenomena, which in this case assume a physiological character. The action of the solar rays is shown to be analogous to that of the ferments.—On a new means of preventing secondary fermentations in the alcoholic fermentations of commerce, by MM. U. Gayon and G. Dupetit. This process consists in adding to the wort antiseptic substances capable of arresting the development of the countless germs contained in the primary substances and in the yeast, without, however, impairing the activity of the leaven itself.—On the alcoholic fermentation of dextrine and of starch, by MM. U. Gayon and E. Dubourg. A new kind of mucor is described, which possesses the twofold property of fixing the water on dextrine, and even on starch, and superinducing fermentation in the products of this saccharification, without, however, affecting cane-sugar or transforming it to alcohol.—On the reduction of the sulphate of copper during vinous fermentation, by M. H. Quantin.—On the genus Cepon, by MM. A. Giard and J. Bonnier. Two new species (*C. pilula* and *C. elegans*) of this little-known genus have been discovered by the authors, the former a parasite of *Xantho floridus*, the latter of *Pilumnus hirtellus*. Both occur on the French seaboard.—On the homologies of the larvæ of Comatulæ, by M. J. Barrois.—On the cave-dwellers of Bêche-aux-Roches, by MM. Marcel de Puydt and Max. Lohest. The authors disclaim all responsibility for the recent remarks of M. de Nadaillac describing the culture of this prehistoric race in somewhat exaggerated language.—On the affinities of the Eocene floras of West France and Saxony, by M. Louis Crié.—On a serious malady analogous to scurvy observed in certain reptiles, by M. Magitot.—On a technical process for the diagnosis of Gonococcus, whereby this parasite may be distinguished from other species of cocci, by M. Gabriel Roux.

BERLIN

Meteorological Society, November 2.—Prof. von Bezold in the chair.—Prof. Spörer spoke of stormy movements in the atmosphere of the sun. He discussed a long series of details respecting the physics of the sun, which were illustrated by heliographic maps, and emphasised the fact that spots invariably appeared only in very hot luminous regions of the solar surface. Of his theoretical explanations it may be more particularly mentioned that, in the opinion of the speaker, the luminous regions originated in the ascending of gases and vapours from the hot interior of the sun. When such a thing happened at the circumference of the sun, then metallic prominences were observed. In consequence of their higher temperature the luminous regions caused ascending currents, whither cooler gases streamed from all sides. The gases, which in certain circumstances mounted to heights of 30,000 German (135,000 English) miles, cooled, sank as cooler masses endowed with greater linear velocity to the same localities, and there formed the sunspots. In the discussion following this address, Prof. Spörer stated that according to his observations the last maximum had shown itself 1884·0. He further stated that occasionally, under special conditions of illumination, he had, with the aid of the telescope, seen in clouds small, round bodies moving up and down, which he had taken for rain-drops, and commended to those interested in the study of the atmosphere such observations of clouds. Respecting the possibility of seeing the rain-drops of clouds in this manner there arose a lengthy discussion.

Physical Society, November 4.—Prof. von Helmholtz in the chair.—Prof. Spörer produced and made the subject of discussion a long series of heliographic maps which he had drawn from phenomena he had himself witnessed, and which demonstrated in a very graphic manner the occasionally very important proper motions of different spots. These self movements always occurred on the west side of the spots, and of the groups of spots. They always followed therefore in the sense of the sun's rotation. They were recognised when the spots were observed several times in the course of a day, and they sometimes attained values of from 1000 to 2000 geographical miles in one day. These movements were specially intense in the case of the formation of larger spot-groups; later on they grew slower. For the explanation of these proper motions, the speaker adduced that sunspots invariably formed themselves only over luminous surfaces, that is, at spots of the solar surface possessing a higher temperature. In his measurements of temperature, which had not yet been published, having reference to the year 1880, he made use of a thermo-element on which, through a fine opening in a thick pasteboard disk, he caused to fall the position of the sun's image which he wanted to measure. According to these observations, the emission of heat from a spot-umbra stood to the radiation of heat from a luminous surface as 10 : 18, and the radiation of a spot-umbra to the radiation of the usual solar surface as 10 : 15. Seeing that the temperatures on the sun stood probably in the same relation as did the radiations, so in the luminous surfaces which possessed a higher temperature (in the relation of 6 : 5) must an ascending gas-current develop, to which a descent of colder gas-masses must necessarily correspond. These descending colder gases it was which generated the spots, and gave them—seeing they possessed a greater linear speed of rotation than did the solar surface—a displacement towards the west in the sense of the rotation.—Dr. Pernet spoke on the determination of the air in the vacuum of the barometer, in accordance with the Arago method, connecting his observations with a publication by Dr. Schreiber, who, on comparing the barometer of the Saxon station with the normal barometer, found, after taking due account of all corrections in the latter, volumes of air far surpassing the permissible quantities. Dr. Pernet had now found that two very essential corrections were overlooked: first, the determinations of the air in vacuum under the pressures 0, 40, and 80 millimetres, were carried out in much too rapid succession, so that compensations of temperature were impossible; second, the effect of the capillarity was not observed, an effect which in the case of syphon barometers played so far a great part, as the lower surface of the quicksilver affected by oxidation and dust had a different surface-tension and different angles of rim from the upper surface of the quicksilver, which was comparatively pure. The registrations were therefore not exact if the menisci were not simultaneously measured. This tension of the surface was in the case of thermometers also very important. In consequence of it, the readings of thermometers with narrow tube and less mass of quicksilver were less exact than the readings of thermometers with wider tube and more quicksilver. It was the cause that thermometers with elliptic tubes were less exact than thermometers with circular ones. The effect of the capillarity, again, was, in the opinion of the speaker, the cause of the "dead point" of Mr. Pickering.

VIENNA

Imperial Academy of Sciences, October 7.—On Hall's phenomenon, by A. von Ettingshausen and W. Nernst.—On the data wanted for proving Avogadro's law, by L. Boltzmann.—On the theory of the electro-magnetic phenomenon discovered by Hall, by the same.—On the density of liquefied methene and liquefied oxygen, by K. Olszewski.—On the comets discovered by Mr. Finlay on September 26, and by Dr. Hartwig on October 6, by E. Weiss.—On colchicine, by S. Zeisel.—Contributions to the knowledge of the Tertiary flora of Australia, second paper, by C. von Ettingshausen.

October 14.—Researches on strychnine, especially on the action of zinc-dust on strychnine, by W. F. Loebisch and P. Schoop.—A preliminary communication on the statistics of comets, by T. Unterweger.

October 21.—To histology and physiology of mucous secretion, by W. Biedermann.—Remarks on L. Hermann's galvanotropic experiment, by E. Mach.—On hydrocarotin and carotin, by F. Reinitzer.—On the anatomy and systematics of gall-mites, by A. Nalepa.

BOOKS AND PAMPHLETS RECEIVED

Encyklopædia der Naturwissenschaften, Erste Abtheil., 48-49 Lief.; Zweite Abtheil., 37-38 Lief. (Trewendt, Breslau).—Index Catalogue of the Library of the Surgeon-General's Office, U.S. Army, vol. vii. (Washington).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. i., part 2 (Cunningham, Sydney).—Quarterly Journal of Microscopical Science, October (Churchill).—Alpine Winter, 3rd edition: Dr. A. T. Wise (Churchill).—Encyclopædia Britannica, vol. xxi. (Black, Edinburgh).—Structure and Life-History of the Cockroach: L. Miall and A. Denny (L. Reeve).—Madagascar, 2 vols.: Capt. S. P. Oliver (Macmillan).—Journal of the Anthropological Institute, November (Trübner).—First Year of Scientific Knowledge, 3rd edition: P. Bert (Relfe).—Nouvel Atlas Céleste: R. A. Proctor; translated into French by P. Gérigny (Gauthier-Villars, Paris).—Ordnance Survey of the United Kingdom: Lieut.-Col. White (Blackwood).—La Photographie sans Objectif: R. Colson (Gauthier-Villars, Paris).—L'Aurora Boréale: M. S. Lemström (Gauthier-Villars, Paris).—Les Hypothèses Cosmogoniques; Examen des Théories Scientifiques Modernes sur l'Origine des Mondes, suivi de la Traduction de la Théorie du Ciel de Kant: C. Wolf (Gauthier-Villars, Paris).—Hand-book of Jamaica for 1886-87: A. C. Sinclair and L. R. Fyfe (Stanford).—Quarterly Journal of the Royal Meteorological Society, October (Stanford).—Monthly Results of Observations made at the Stations of the Royal Meteorological Society, vol. vi., No. 22 (Stanford).

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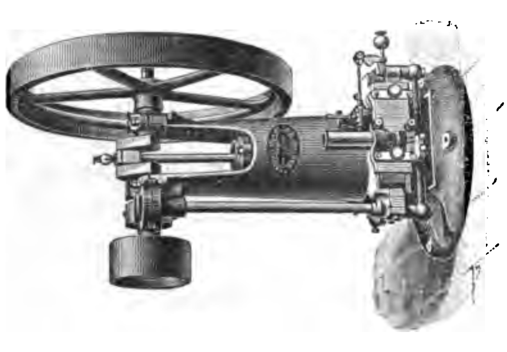

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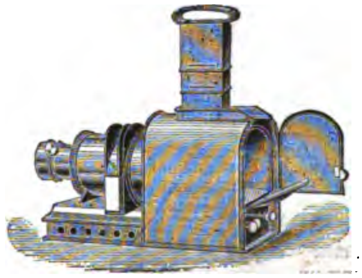

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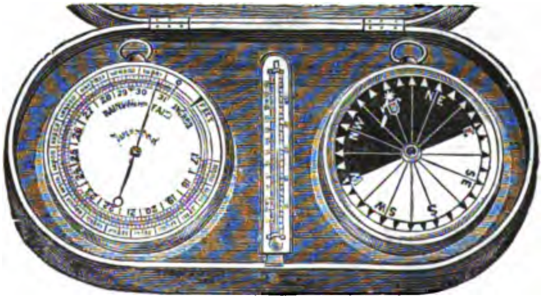

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THURSDAY, NOVEMBER 25, 1886

EXPLORATION OF THE NORTH SEA

Die Ergebnisse der Untersuchungsfahrten S.M.Knbt. "Drache" (Kommandant Korvetten-Kapitän Holzhauer) in der Nordsee in den Sommern 1881, 1882, *und* 1884. Veröffentlicht von dem Hydrographischen Amt der Admiralität. (Berlin: Ernst Siegfried Mittler und Sohn, 1886.)

OUR knowledge of the physical conditions of the North Sea has just been enriched by the publication of the results of the expeditions of the Prussian ship *Drache* during the summer months of the years 1881, 1882, and 1884. The expeditions and the publication have been carried out under the direction and with the authority of the Hydrographic Office of the German Admiralty.

Prof. Möbius, who has examined the organisms collected by the *Drache*, reports that he has found nothing worthy of special mention among the biological collections. It is otherwise with the physical and chemical observations, for the whole of the volume before us is devoted to these observations, their analysis and discussion. The publication is accompanied by synoptic tables showing the positions of the observing stations and the scientific results, as well as by fourteen charts setting forth graphically the currents, the depths, the salinity, specific gravity, and the quantity of oxygen in the surface, intermediate, and bottom water, and sections illustrating the distribution of temperature.

The temperature and salinity are first examined. The observations confirm the view that the salt heavy water of the Atlantic enters the North Sea by the north of Scotland, and, on being cooled, sinks to the bottom, and fills all the deeper parts of the basin, including the Norwegian Gut. The observations of the Norwegians and those on board the *Triton* showed that, in like manner, the deep water of the Norwegian Sea was largely made up of the salt Atlantic water, which sank to the bottom on reaching a colder latitude—probably mixing much with deep colder Polar and fresher water. The *Drache* traced this salt Atlantic water to the centre of the North Sea. It would be a matter of very great interest to have the temperature of the water taken at stated intervals throughout the year in the Norwegian Gut, in a similar manner to the observations now being carried on in the deep lochs of the west of Scotland. The observations on the currents of fresher water running to the north along the coasts of Britain and Jutland—the latter eventually meeting and mixing with that of the Baltic—are very interesting. Indeed, the extensive current and tidal observations are valuable additions to knowledge; but, as the author remarks, both they and the temperature observations are incomplete, being confined to the summer months, and he indicates the regions where observations are much required. Still, combined with the winter observations which we possess at certain points, the *Drache's* observations greatly augment our knowledge of the physical conditions of the North Sea, and of the modifying influences produced by the seasons.

The chemical work has been intrusted to Dr. Neumeister, under the direction of Prof. Jacobsen, and the geological part is by Dr. Gümbel. The chemical work includes the determination of the oxygen and nitrogen in water from different depths. Dr. Neumeister found in surface-water (mean of twenty-five analyses) the oxygen to be 33·95 per cent., the volume of the sum of the oxygen and nitrogen equalling 100. In deep water (200 metres) the oxygen descended to 25·20 per cent. of the volume of the two gases.

For carbonic acid combined as neutral salts, he found for surface-waters 52·66 milligrammes per litre (mean of sixty-seven determinations); the partially combined acid was found to be 43·78 milligrammes (mean of thirty-nine determinations).

As appendix to these researches, the results are given of the determinations of the carbonic acid in the waters of the Atlantic, Indian, and Pacific Oceans, collected by the *Gazelle* in 1874-76. The carbonic acid combined as neutral salts in the surface-waters reaches to 52·5 milligrammes per litre (mean of thirty-one observations). At 183 metres of depth, the mean is 53·2. For greater depths, down to 5000 metres, fourteen determinations gave 50·6 to 56·8 milligrammes. Four determinations gave 59 to 70 milligrammes, and one gave 82·7 milligrammes. No attempt is made to compare these with the *Challenger* results.

The author explains the presence of the large quantity of carbonic acid in deep water by the fact that the water dissolves the carbonate of lime, which is found in great quantity on the bottom in all moderate depths. The carbonic acid which effects this dissolution is probably furnished by the oxidation of organic substances. The author refers to the fact that carbonic acid is not necessary in order that carbonate of lime may be dissolved by sea-water, and has, in this respect, confirmed Dittmar's observations. Different waters, however, comport themselves very differently in this respect. The water of great rivers, adds the writer, at their embouchure contains less acid combined as neutral salts than ocean water, and the mixture of salt and river water, along coasts, less carbonic acid than the water in the great oceans; but the difference is not in proportion to the quantity of salts present. It is shown by analyses of Baltic water that while this water contains only about one-half of the salts present in pure ocean water, it contains nearly nine-tenths of the carbonic acid present in the neutral salts of pure ocean water.

Gümbel's work consists in an examination of the deposits collected from depths ranging from 18 to 317 metres. The forty samples, of which an excellent description is given, all belong to littoral, sub-littoral, or terrigenous deposits. None of them present the essential characters of truly deep-sea or pelagic sediments. The author divides them into quartz sands and sandy clays, the latter being of a much darker colour than the former. Gümbel has followed in his descriptions the methods indicated in the preliminary notices of the *Challenger* deposits. Gümbel attributes the absence of Globigerina ooze from the samples to the relatively shallow depths from which they were procured, and he adds that the depth determines the nature of the deposit. This is quite a mistake: it is, rather, distance from land that determines the kind of deposit. Deposits not unlike those described by Gümbel occur in depths of over 2000 fathoms when near to land, while a Globigerina ooze or Pteropod ooze may occur in very shallow depths, in the tropics, far from land. These deposits of the *Drache*, being near the coast, it is found that quartz predominates. The fragments of plagioclase, orthose, hornblende, augite, bronzite, mica, garnet, tourmaline, dicroite (is it not glaucophane ?), magnetite, zircon, chlorite, all come from the disintegration of the ancient rocks which form the coast of Norway and Scotland. Gümbel also finds fragments of granitic rocks, dioritic rocks, &c. Fragments of modern volcanic rocks, such as lavas and pumice, are very rare when compared with the particles derived from ancient rocks. Glauconite was found in some of the specimens, and the author believes that these have been transported, which is quite unlikely, as large deposits of glauconite are now in process of formation along the coasts of the north of Scotland. The organisms—mollusks, echinoderms, foraminifera, and diatoms—are all the same as those usually found in partially inclosed seas like the North Sea, and do not present any peculiarities worthy of note.

The author supposes that there is a continuation under the North Sea of the ancient rock-masses of Scandinavia. This may be true, but the supposition can in no way have been suggested by the chemical, microscopic, and mineralogical examinations of the deposits of the North Sea. In conclusion, Gümbel states that the sediments of the North Sea prove that sandy deposits can be formed alongside of clayey and marly deposits, during the same time in the same sea. This conclusion has already been perfectly established, and this confirmation supports an interpretation generally received, which was one of the first results of the examination of the *Challenger* deposits.

The Hydrographic Office of the German Admiralty have done excellent service in taking up the scientific examination of the North Sea. It is a work that we would like to see continued and advanced by our own Hydrographic Office. J. M.

OUR BOOK SHELF

Chemical Arithmetic. By Sydney Lupton, M.A., F.C.S., F.I.C. Second Edition. (London: Macmillan and Co., 1886.)

WE are pleased to note a new edition of this excellent work, in which several improvements have been made. The hundred pages of introductory matter in the first edition have been reduced by about one-half, much unnecessary pure arithmetic having been cut out. The 1200 examples with answers are, on the whole, well selected, though many of them can scarcely be called chemical. A greater number of typical examples might advantageously have been worked out at full length.

The book is especially to be commended for its clear and concise definitions, which are in many books very loosely expressed. The differences between density and specific gravity, atomic and molecular weights, for instance, are explained in a manner that any student of ordinary ability will readily understand. We feel sure that the book will be appreciated alike by students and teachers, but it will be especially valuable to teachers.

Experimental Chemistry. By C. W. Heaton, F.I.C., F.C.S. New Edition, Revised. (London: George Bell and Sons, 1886.)

ANOTHER edition of this work on experimental chemistry, adapted from the German of Dr. Stöckhardt, has just been issued. To those students of limited means who desire to work at chemistry as well as to read it—and it is for those that the book is intended—it will be found useful. The introduction, however, is much too extensive and theoretical for beginners, and we fear that many would be disheartened before reaching the really experimental work. In our opinion, the book is not sufficiently practical, many experiments lacking detail. We would suggest that in future editions a few pages be devoted to instructions in the manipulation of apparatus and the working of glass.

Part IV., which is devoted to organic chemistry, is very clearly set out. The book is not sufficiently modernised for these days of competitive examinations, but the teacher who is desirous of encouraging his students to perform simple experiments in spare moments would get many valuable ideas from it.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Sense of Smell

IN your issue of Septhmber 30 (vol. xxxiv. p. 521) your correspondent Dr. Arthur Mitchell is desirous of obtaining some data in regard to the sense of smell. In a paper presented at the Philadelphia meeting of the American Association for the Advancement of Science (1884) we have described a series of experiments designed to test the delicacy of this sense. These experiments, being of a preliminary character, have hitherto been withheld from publication, but the following brief statement of the results obtained may be of interest to Mr. Mitchell and to other readers of NATURE. We made use of the following substances:—(1) oil of cloves, (2) nitrite of amyl, (3) extract of garlic, (4) bromine, (5) cyanide of potassium. A series of solutions of each of these was prepared, such that each member was of half the strength of the preceding one. These series were extended by successive dilutions till it was impossible to detect the substances by smell. The order of the bottles containing these solutions was completely disarranged, and the test consisted in the attempt to properly classify them by the unaided sense of smell. The thirty-four observers who assisted in these experiments were of both sexes; the results are indicated in the following table (I.):—

Amount detected

	Oil of cloves	Nitrite of amyl	Extract of garlic	Bromine	Cyanide of potassium
Average of 17 males	1 part in 88,218 of water	1 in 783,870	1 in 57,927	1 in 49,254	1 in 109,140
Average of 17 females	1 part in 50,667 of water	1 in 311,330	1 in 43,900	1 in 16,244	1 in 9,002

The same method of investigation has since been followed by one of us[1] in some experiments, the results of which are given in Table II.

Amount detected

	Prussic acid	Oil of lemon	Oil of wintergreen
Average of 27 males	1 part in 112,000 of water	1 in 280,000	1 in 600,000
Average of 21 females	1 part in 18,000 of water	1 in 116,000	1 in 311,000

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[1] "Some Special Tests in Regard to the Delicacy of the Sense of Smell," by E. H. S. Bailey and L. M. Powell (*Proc. Kansas Acad. o Science*, vol. ix.).

Many striking individual peculiarities were noticed in the course of the experiments, which these general averages fail to show. Three of the male observers were able to detect one part of prussic acid in about 2,000,000 parts of water. Two of these were persons engaged in occupations favouring the cultivation of this sense. Careful chemical tests failed to show the presence of prussic acid in several of the more dilute solutions, in which it could be detected by the sense of smell. We found some of both sexes who absolutely could not detect prussic acid even in solutions of almost overpowering strength. There were several instances of the same peculiarity as regards bromine. Again, our averages show that the sense of smell is in general much more delicate in the case of male than of female observers.

EDWARD L. NICHOLS
E. H. S. BAILEY

University of Kansas, November 4

Tidal Friction and the Evolution of a Satellite

ADVERTING to the correspondence in NATURE (vol. xxxiv. p. 286), I think that Mr. Darwin has not, so far, fully realised the results that would follow from the circumstance that the Martian satellite's period would be affected many hundred times more than that of the planet's rotation, as explained in the correspondence referred to. He argues that, the moon's mass being great, she should recede to an enormous distance before there will be a reversal of the direction of her tides on the earth; while the satellite of Mars, being very small, need only to recede a short distance before a similar tidal reversal ensues. No mention being made of any other supposed difference in the systems at the starting-point, it must be inferred that other things are supposed about equal. But, as a matter of fact, the present position of the Martian satellite is incompatible with an initial rotation of its planet anything like so great as that ascribed to the earth at a like stage. If Mars be supposed to rotate ten times while the satellite, at its present distance, makes nine revolutions, the satellite's period would still be affected or lengthened much more than would that of the planet's rotation. The difference between the periods of revolution of the planet and satellite would increase quickly at first, but more slowly as the satellite receded a certain distance, till at a certain time there would be no increase, after which there would be a decrease, and finally a reversal. When the satellite would have receded to a short distance, where she would revolve in the same period as Mars now rotates in, the planet would have lost but little of its original rapid rotation. Now, supposing the satellite tide to go round in the same time as the solar one, the period of the satellite would be affected about thirty times as much as that of the planet's rotation. Allowance being made for the comparative slowness of the satellite's tides, the satellite's period would still be changed more than ten times as much as that of the planet. It would be only when the little body got further out, and the planet's rotation slower than it now is, that there could be a reversal of the direction of the satellite's tides. Wherever started, the satellite must either go directly into the planet, or go out a short distance and back into the planet, before the rotation-period can have been much changed by solar tides; or else the satellite must go far out—as when it gets a fair start—and could not possibly turn back until the rotation of Mars be slower than now. Hence it seems that under no conditions could the rotation of Mars, at the birth of her moon, have been twice as rapid as now, and the evidence is very strong that the rotation-period could not have been changed more than a very few hours, if so much. Then, if the rotation of Mars was so slow in the beginning, and so little changed during the whole existence of the satellite, the circumstance does not support the view that the earth's rotation was very rapid in the beginning and so much changed during her past history, but rather inclines the other way.

Respecting the statement that two heavenly bodies cannot revolve about their centre of inertia as parts of a rigid body with their surfaces nearly in contact, unless one be smaller and denser than the other by a certain amount, I can only say, at the present time, that such was the conclusion at which I arrived when investigating the results of the tidal effects of two bodies on one another at close quarters. Without going far into the question, it can be seen that if the rule holds when the two bodies are of the same size and density, it will hold throughout. There will be no difficulty in seeing that the rule holds so far that when the difference in size between the bodies is as great as between any of the satellites and its primary, the small body must be invariably the denser. Now the argument that was supposed to apply in general would at least apply in the case of the solar system. That argument, as explained in my pamphlet, was that, if a rapidly-rotating body were to separate into two, the small body given off must be denser than the other to withstand the tidal disturbance, and that it would be impossible for the small body to be denser than the primary, since the secondary body must be formed from the surface and therefore lightest part of the other body.

JAMES NOLAN

Dergholm, Victoria, October 5

Seismometry in Japan

I HAVE read, with no small surprise, a paragraph in NATURE of November 11 (p. 36), giving a summary of a letter from Prof. John Milne, with reference to an article by me on the seismographs now manufactured by the Cambridge Scientific Instrument Company. Prof. Milne is represented as saying that, "with the exception of one or two which have been modified, a set of instruments like those recommended by Prof. Ewing are, so far as Japan is concerned, quite obsolete." His letter is not published, and it is possible that the paragraph inadvertently does him an injustice in making him assert what has absolutely no foundation in fact.

In any case the statement cannot be allowed to pass without contradiction. My seismographs have been in regular use at the University of Tokio since they were invented; they are now used for systematic observations by the Japanese Meteorological Bureau; they were sent last year by the Japanese Government to the Inventions Exhibition in London, where they were awarded the highest diploma among Government exhibits: one of them, the comparatively cheap and simple duplex pendulum seismograph, is employed by many private observers in Japan. In a letter received only a few weeks ago, my friend and former assistant, Mr. Sekiya, now Professor of Seismology in the University, says:—

"We are going to start a journal called the *Journal of the Science College of the Imperial University, Japan*. In the first number I will give a paper on 'Comparison of Earthquake Diagrams simultaneously obtained at the same station by two instruments involving the same principle, and thereby proving the trustworthiness of these instruments.' Of course I treat those diagrams recently obtained by two of your seismographs."

Other letters from Prof. Sekiya are full of accounts of the excellent work he is doing with these instruments, and of their continued and extended usefulness in his very able hands. A paper lately received from him describes a rough but effective form of the duplex pendulum, cheaply made in order to bring it within the reach of private observers, and with reference to this the *Japan Mail* of February 2, 1886, says:—

"The duplex pendulum seismograph designed by Prof. J. A. Ewing, has been employed for earthquake observations in the Tokio Daigaku by Mr. S. K. Sekiya, who has improved many of its details during his long use of the instrument. On account of the simplicity and scientific nature of its construction, and its easy management, it has found its way into the hands of many observers."

The *Mail* goes on to mention the name of a native firm by whom the instrument is made and sold. In March last Mr. Sekiya writes:—"The duplex pendulum sells well; some fifteen or twenty of them have been sold."

So much for the duplex pendulum seismograph, which is one of those described in my article, and now made with the utmost refinement of construction by the Cambridge Company. The other is a three-component instrument, of which the principal part is the horizontal pendulum seismograph—consisting of a pair of horizontal pendulums for recording separately two rectangular components of the horizontal motion of the ground on a moving surface driven by clockwork. This method of recording earthquakes was introduced by me in 1880 (*Trans. Seis. Soc. Jap.*, 1880; *Proc. Roy. Soc.*, No. 210), and has been in regular use ever since. The instruments made to my designs by native workmen are still doing good service in Prof. Sekiya's hands. Those now made by the Cambridge Company have the advantage of better workmanship and an improved arrangement of parts. As Prof. Sekiya has recently written to me with regard to the purchase of a set of them by the Japanese Government, it is probable that Mr. Milne will before long have an opportunity of seeing the latest forms of the instruments in Japan.

No one knows better than Prof. Milne that the horizontal pendulum seismograph is not obsolete. He adopted it himself soon after I introduced it, and he has used it freely in his own investigations. His letter will be understood to mean that since I left Japan in 1883 there has been a new departure in seismometric methods which has made my apparatus fall out of date. There has been nothing of the kind. Can Mr. Milne point to any methods involving novel features of importance, and say what their novel features are? It would be odd for instruments to become obsolete when they answer their purpose very well, and when there is nothing better to take their place.

J. A. Ewing

University College, Dundee, November 13

Ozone Papers in Towns

I take the opportunity of mentioning that I have experimented with Moffatt's ozone papers in London for the past month, and find that on exposing the papers already previously coloured they all become bleached to their original white. They were previously exposed to the air at Brighton and Hastings, on the sea-coast, and were then coloured, and afterwards preserved closely shut up for trial in the mephitic air of towns.

Some more stained papers were also received from Cheltenham, which also became blanched on open-air exposure in London, though as highly stained as 8 degrees. They were not washed by rain, but kept dry in the usual cage in the open air and out of the sun; and they were of various shades of colour, from 2 to 8, as already marked on them. I should like to know or ask for opinion as to the chemical changes that had taken place, and if these had been due to an antozone causing a recomposition of the ingredients (starch and iodide potassium) to their original constitution. It may be likely, therefore, that in Moffatt's papers, coloured previously, we may have the means of testing the impure condition of the air of any locality by exposing them in it for a few hours.

Other papers had already been prepared for testing the sulphurous impregnation of the town air, as by compounds of lead, tin, &c.; but, though they became stained in the laboratory, yet they failed on trial in the open air. As to the influence of the wind, the quickest effect seemed to be produced by easterly winds, while those from the south-westerly direction were slower in action on the papers; but this, I think, may be merely due to the air from the east in London blowing first over a greater expanse of city, carrying with it adulterating emanations.

W. J. Black

London, November

The Similarities in the Physical Geography of the Great Oceans

In the abstract of my paper read at the Royal Geographical Society on the 8th inst., which was published in Nature of Nov. 11, there is a statement (p. 34) that the weight of the column of water between 20 fathoms and 70 fathoms from the surface under the westerly equatorial current is only 88 per cent. of the weight of the same column under the easterly counter equatorial current. I regret that a serious arithmetical error occurs in the calculations on which this statement was founded. There is no such considerable difference of weight in the two columns of water.

J. Y. Buchanan

Edinburgh, November 22

Lung Sick

Dr. E. J. Dungate, with compliments to the Editor of Nature, begs to inclose him a letter which he has just received from Prof. Smets, of Hasselt. It refers to the letter on "lung sick," which appeared in Nature for November 11 (p. 29), and contains most important evidence on the subject. Dr. Dungate is sure, from the genial tone of the letter, that the Editor of Nature is at liberty to publish it, if he desires.

6, Marchmont Road, Edinburgh, November 17

Hasselt (*Belgique*), *le* 14 *Novembre*, 1886

Monsieur Dungate,—J'ai lu votre demande dans la Nature du 11 Novembre.

L'inoculation préventive de la pleuropneumonie exsudative a commencé à Hasselt, et la méthode, suivie déjà chez les Zoulous, a été préconisée, en premier lieu, par un médecin de Hasselt, M. le Dr. Willems. Je vous communiquerai, avec plaisir, ses travaux si vous les désirez.

On a essayé, à diverses reprises, à Hasselt, les inoculations au fanon, à la poitrine, etc.; elles ont eu des conséquences mortelles.

Je crois que cette pratique permettait au microbe d'envahir rapidement les poumons, et d'étouffer le bœuf. Mais quand on pratique l'inoculation à la queue, le microbe a passé par les divers stades de son existence, et est déjà à son déclin avant d'arriver au poumon. Il est possible aussi que le microbe, que je crois fortement aérobie, a été atténué dans sa virulence par suite de son passage dans des organes où l'oxygène est plus rare

Plusieurs fois, néanmoins, l'inoculation est encore mortelle, en moyenne 1 cas sur 100 inoculations à Hasselt.

La perte de la queue est due, à mon avis, à ce que l'on fait usage d'un virus impur, obtenu empiriquement, sans culture. La gangrène, qui emporte une partie de la queue, peut être causée par un autre organisme inoculé simultanément avec le microbe de la maladie. Car, parfois, plusieurs bêtes inoculées en même temps, avec le même virus, ne perdent pas la partie inférieure de la queue, tandis que d'autres fois cet accident est fréquent. On prévient partiellement cet accident en faisant des incisions longitudinales dans l'engorgement qui se produit.

Agréez, Monsieur Dungate, l'expression de ma considération distinguée et de mon entier dévouement.

Dr. Gérard Smets,
Professeur à Hasselt (Belgique)

Meteor

Passing along Kensington Gore yesterday at 7.20 p.m., I saw the finest meteor I have ever seen in my life. It descended from near the zenith perpendicularly through the constellation of the Great Bear. It was much larger than any planet. About half-way on its downward course it gave out a second meteor of a red colour, being itself of a pale yellow. The atmosphere was rather foggy at the time, but I could see the stars through the mist. It was, no doubt, the same meteor as is mentioned in to-day's *Times* as having been seen at Reading.

P. L. Sclater

3, Hanover Square, London, W., November 18

The Origin of Species

Mr. Catchpool, writing in Nature (vol. xxxiv. p. 617) on this subject, says:—"If B is separated from A by being nearly infertile, and C from B in the same way, C is likely to be still more infertile with A." This is quite a mistake. Suppose B to be the cat species, and A and C two varieties of dogs; A and C are quite fertile with each other, and infertile with B.

It is certain that mutual infertility is not caused by mere visible unlikeness. The horse and the ass, which do not produce fertile offspring, are much less visibly unlike than many of the varieties of dogs or of pigeons, which are mutually quite fertile.

May not mutual infertility be a result of long-continued separation, quite independently of any unlikeness arising? I do not know whether this conjecture is supported by any observations on the mutual relations of kindred species or varieties in lands separated by oceans.

Joseph John Murphy

Belfast, November 8

Mr. Murphy has mistaken my meaning, which I will try to make clear by an example. Suppose one brood of an ancient species of Gallinæ to have exhibited, as a sport, a partial infertility with the rest of the species, while the birds composing the brood remained abundantly fertile among themselves. Suppose the main body of that species to have become, by natural selection, our pheasants, while the isolated brood became the ancestors of our grouse. Suppose one brood of these grouse to have become partially infertile with the main body of grouse, and to have been the ancestors of our red grouse, while the main body of the grouse became, by natural selection, our black grouse. If, as I believe, variation does not produce or increase infertility, the black grouse will still be only partially infertile with the pheasant, and the red only partially infertile with the black grouse; but it seems probable, *primâ facie*, that the second spontaneous infertility would remove the red grouse further from the pheasant, so that these would be quite infertile. But this is merely argument from analogy; there is no evidence of the result of such superposed "sports," and retrogression to greater fertility seems possible.

This instance is not a good one, because the observed partial infertility (*i.e.* only occasional fertility) between pheasant and black grouse may be due to dislike, not partial impotence. But I doubt whether distaste for pairing and impotence when paired are often quite dissociated.

Mr. Murphy asks, as I asked in these pages in 1884, and others have asked since, for one simple fact which would be decisive. Is it, or is it not, the fact that allied species which are confined each to a particular island, prove, when brought together, far less frequently infertile than species, equally dissimilar, which had lived in the same district, might be expected to prove. On the answer to this question depends, as far as I can see, the fate of the theory of physiological selection. Can no one answer it?

EDMUND CATCHPOOL

THE CORAL REEFS OF THE SOLOMON ISLANDS[1]

OUT of a collection of nearly seventy corals which I made in these islands, nearly a quarter are new or undescribed; and from this fact, as I am informed by Mr. S. Ridley, it may be inferred that there is yet much to be learned of the corals of this region. After describing in my paper the characters of a typical reef, I proceeded to refer to the complex relations that exist between the multitudes of creatures that frequent coral reefs. The protective colouring of the small crabs that live among the branching corals often attracted my attention. I recall, in particular, the instance of a small crab that finds its home among the branches of a *Pocillopora*. The light purple colour of its carapace corresponds with the hue of the coral at the base of the branches, where it lives; whilst the light red colour of the big claws, as they are held up in their usual attitude, similarly imitates the colour of the branches. To make the guise more complete, both carapace and claws possess rude hexagonal markings, which correspond exactly in size and appearance with the polyp-cells of the coral. Another species of crab, that climbs about the blue-tipped branches of a *Madrepora*, has the points of its pincer-claws similarly coloured. It is interesting to note that these two crabs are adapted to live each on its own species of coral. Had I caused them to exchange their homes, their borrowed hues and markings would have at once made them conspicuous objects for their enemies.

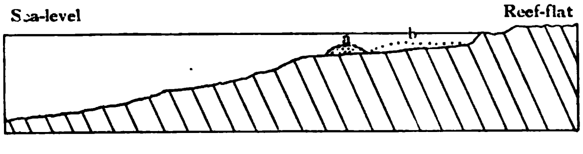
I paid especial attention to the inter-tidal exposure of living corals, and was much surprised at the number of species which are bared by the ebbing tide. Of all the corals in these islands, those belonging to the genus *Cœloria* seem to be the hardiest in this respect. In the paper I have described my observations with some detail.

Coral Reefs and Shoals.—The earliest condition of the coral reefs in this group is to be found in that of the numerous detached submerged reefs or shoals which lie below the limit of the constructive power of the breakers, having been arrested in their upward growth at depths varying between 5 and 10 fathoms according to the exposed or protected character of their situation. This remarkable fact of the arrest of the upward growth of the coral at these depths was utilised by Lieut.-Commander Oldham whilst surveying these submerged reefs in H.M.S. *Lark*. If a shoal was not marked at the surface by a reef-flat or by an islet, we could sail over it with perfect safety. The broken water of the tide-rip that indicated these shoals was no source of danger for a vessel of light draught. In my paper I have given evidence to prove that a shoal which was found by Bougainville in 1768 to be covered by 5 fathoms, remains in the same condition at the present day. The number of coral shoals possessing these characters led me to the conclusion that isolated submerged reefs are unable without the assistance of a movement of elevation to raise themselves to within the constructive power of the breakers. When they have reached their upward limit, they extend laterally, forming ultimately flat-topped shoals. It may appear bold to suggest that atolls and barrier-reefs owe their appearance at the surface to a movement of elevation; but we know that in the regions occupied by the atolls of the Low Archipelago, of the Fiji Islands, and of the Pelew Group, the last movement experienced has been one of elevation; whilst the observations of Mr. Beete Jukes on the Australian Barrier-Reef go to show that, if there has been a recent change of level in that region, it was one of the same nature. In the atoll of Oima in the Solomon Group I found evidence of an anterior elevation.

In my paper I proceeded to describe at some length the reefs that have reached the surface. In this abstract, however, I can only refer to the fact that all the three classes of reefs are to be found in this group; the atolls, I should add, being comparatively few in number and of small size.

The Formation of Atolls.—My observations go to show that atolls of small size (a mile or two across) do not assume their characteristic form until they have reached the surface. After upheaval has brought a submerged coral shoal within the constructive power of the breakers, it soon appears at the surface as an isolated patch of reef. Extensions or wings grow out on either side, and, guided by the prevailing currents (in the manner described by Semper), they ultimately form the common horse-shoe reef, which presents its convexity against the currents. Large atolls evidently begin to assume their characteristic shape below the surface as described by Murray and A. Agassiz.

The Formation of Barrier-Reefs.—The facts on which my conclusions have been based were obtained by the examination of the weather slopes of reefs. For the first 70 or 80 yards from the weather edge of a reef there is a gradual slope, largely bare of living coral, to a depth of 4 or 5 fathoms. There is then a rapid descent to a depth varying between 12 and 18 fathoms. It is this declivity that constitutes the growing edge of the reef, and the sand and gravel produced by the constant action of the breakers collect at its foot. When the submarine slope is more than 10° or 12°, as is usually the case, the sand and gravel extend far beyond the depths in which reef-corals thrive; but when the slope is gradual, *i.e.* less than 5°, the lower margin of this band of detritus lies within the reef-coral zone, and in consequence a line of barrier-reef is ultimately formed beyond this band with a deep-water channel inside (*vide* diagram). Should the area be undergoing elevation, a succession of concentric lines of barrier-reefs will originate, line after line being advanced as fresh portions of the sea bottom are brought towards the surface, each line growing upward along the lower margin of the band of detritus derived from the line of reef inside it. In such a manner have the Shortland Islands been produced. When I arrived at the above conclusion I was not aware that substantially the same explanation had been advanced thirty years before by



Barrier-reef of Choiseul Bay (drawn on a true scale to the 100-fathom line. *a* = incipient barrier-reef (size purposely exaggerated); *b* = belt of sand and gravel.

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[1] Abstract of a Paper by H. B. Guppy, M.B., late Surgeon H.M.S. *Lark*. Communicated by Dr. John Murray to the Royal Society of Edinburgh on July 5, 1886.

Prof. Joseph Le Conte in the instance of the Florida reefs. He then pointed out that since corals will not grow on muddy shores or in water upon the bottom of which sediment is collected, the favourable conditions can only be obtained at some distance from the shore, where a barrier-reef would ultimately be formed *limited on one side by the muddiness and on the other by the depth of the water.*

The foregoing conditions may be described as the determining causes of a barrier-reef. After the reef has been formed, the lagoon-channel will be kept open by such agencies as solution, diminished food-supply, tidal scour, organic degradation, and other influences. The circumstance that barrier-reefs are frequently situated at or near the borders of submarine plateaus receives a ready explanation in the view first advanced by Prof. Le Conte, since in such situations the necessary conditions of depth and clearness would be found.

Anomalous Depths of some Atolls and Barrier-Reefs.—One of the principal arguments in favour of the theory of subsidence lies in the assertion that lagoons and lagoon-channels are sometimes deeper than the reef-coral zone. I will, however, endeavour to show that this assertion is founded on a misconception of the conditions that limit the depth of this zone. The extent to which the depth may vary is demonstrated in the great divergence between the estimates of different observers in every region of coral reefs. Those of Quoy and Gaimard, Ehrenberg, Darwin, Dana, Murray, A. Agassiz, and others, range from 5 to 40 fathoms. But this variation may also be found in the same region of coral reefs. Thus, in the Solomon Islands, I found that the depths at which reef-corals flourished ranged in different localities from 12 to 40 fathoms and beyond, the variation being due to differences of local conditions, such as the degree of inclination of the submarine slope, the presence and position of submarine declivities, the amount of sediment held in suspension, the force of the breakers, and other influences. The main determining condition, as Prof. A. Agassiz points out, is to be found in the injurious effect of sand and sediment rather than in the general influence of depth; and the distribution of these materials is dependent on the local conditions above referred to. Local conditions will usually restrict the reef-coral zone to depths less than 30 fathoms; but, where there is a gradual submarine slope, reef-corals are to be found in depths beyond the sand and gravel. Inasmuch as most observers have regarded these materials as necessarily limiting the zone, they did not push their inquiries beyond. Under favourable conditions, however, reef-corals may thrive in depths of 50 or 60 fathoms; and thus we can readily explain the apparently abnormal depths inside some atolls and barrier-reefs.

An apparent objection here presents itself. If reefs begin to build their foundations in depths greater than those which are generally assigned to them, the thickness of the elevated reef-formations discovered by me in the Solomon Group should have been much greater than 150 feet, the actual limit of their thickness. It will, however, have been gathered from the previous remarks that local conditions will usually confine reef-corals to depths less than 25 or 30 fathoms, and that it will be only under occasional circumstances that reefs will commence to be formed in deeper water. Fringing-reefs themselves are at first restricted to shallow waters around the coast, and their seaward extension in localities where the submarine slope is at all steep, as is generally the case, must be extremely slow. Again, in an area of elevation, such as that in which the Solomon Islands are included, barrier-reefs, which may have begun to grow in depths not less than 50 fathoms, might owe their approach towards the surface as much to the elevating movement as to the very slow upward growth of the corals. It should also be borne in mind that the rapid subaërial denudation, to which these regions of heavy rainfall are subjected, would be an important agency in the thinning away of the raised coral formations.

In the latter part of my paper I refer, amongst other subjects, to the extensive character of the degradation of coral reefs by multitudes of organisms. I also give proofs of the outward growth of reefs on their own talus (as described by Murray)—(1) in the circumstance that massive corals may be commonly observed to increase in size as one approaches the lagoon from the outer margin of the reef-flat; (2) in the presence of old lines of erosion evidently produced at the existing sea-level, but which have been cut off from the action of the waves by the advancing edge of the reef-flat; (3) in the characters and position of the wooded islets situated on reefs, which in course of time would cover the whole reef-flat, were it not for one counteracting circumstance, the seaward growth of the reef.

Lastly, I refer to the deposits at present forming on the outer slopes of reefs in depths down to 100 fathoms. Reef-debris, foraminiferous tests, especially of *Orbitolites*, joints of the calcareous alga *Halimeda opuntia*, portions of *Nulliporæ*, and the small detached corals of the genus *Heteropsammia*, enter largely into the composition of these deposits. I should add that a rock of this composition is one of the commonest types of the so-called coral limestones in the Solomon Group.

In this short abstract of a long paper I have not been able to do much more than indicate the general bearing of my conclusions. The facts and data are given at length in the original paper.

THE BRITISH ASSOCIATION AND LOCAL SCIENTIFIC SOCIETIES

THE second annual Conference of Delegates held under the new rules of the British Association met at Birmingham on September 2 and 7, in the library of the Medical Institute. Forty-nine local Societies carrying on work in various parts of the United Kingdom have been enrolled this year as "Corresponding Societies" of the Association, and of these thirty-two were represented by Delegates at the Birmingham meeting. The following report of the proceedings of the Conference, signed by Mr. Francis Galton and Prof. R. Meldola, the Chairman and Secretary of the Committee, has just been circulated among the Corresponding Societies, and it will be seen that this new branch of the work of the Association promises to be of mutual advantage both to the Societies and the Association:—

At the first Conference the chair was taken by Dr. A. W. Williamson, F.R.S., General Treasurer of the British Association, the Corresponding Societies Committee being represented by Captain Douglas Galton, F.R.S., General Secretary of the Association, Dr. Garson, Mr. John Hopkinson, F.L.S., and Prof. R. Meldola, F.R.S., Secretary.

The Secretary read the Report of the Corresponding Societies Committee which had been presented to the Council of the Association.

The Chairman made some remarks explanatory of the objects of the Conference of Delegates, and suggested that among other subjects of investigation in which it might be useful to secure the co-operation of the local Societies was that of injurious insects, already so much studied by Miss E. A. Ormerod.

The Secretary also made some observations in explanation of the constitution of the Corresponding Societies Committee and the relations existing between the Conference of Delegates and the British Association.

Some remarks were made by Mr. J. W. Davis and others with reference to the advisability of securing the co-operation of the local Societies for the purpose of investigating British Barrows and other prehistoric remains. This suggestion had been put forward at the Aberdeen Conference last year by Prof. Meldola, and a Committee was about to be formed by Section H for carrying out this object.

Mr. H. Heywood considered that the relationship now existing between the British Association and the Corresponding Societies had already been of great assistance to the Societies themselves. In the case of his own Society (Cardiff) they had been able to assist one of the Committees (Erratic Blocks) brought under the notice of the Aberdeen Conference last year.

Prof. Lebour stated that many of the local Societies, such as the North of England Institute, which he represented, were composed of engineers connected with large works, who might make useful investigations which would be facilitated if backed up by the authority of the British Association. For this reason he hoped that other subjects besides natural history, geology, or anthropology would be recognised at the Conferences.

Captain Galton explained that the object of the Conference of Delegates was to bring the Corresponding Societies into direct communication with *all* the Committees of the British Association, to which the local Societies or individual members of these might render assistance. This could of course be only effected by degrees, but he suggested that as a preliminary step it might be found useful to place the Delegates on the Committees of those Sections in which they or their Societies had the most interest.

Dr. Williamson supported this proposition, and the Secretary took down the names of the Delegates to be attached to the various Sectional Committees.

Prof. Hillhouse and Dr. Garson expressed their willingness, as Secretaries of Sections D and H respectively, to propose Delegates as members of the Sectional Committees.

Mr. Hopkinson suggested that among other methods of promoting work among local Societies it might be found advantageous for the Delegates themselves to make suggestions at the Conference which might lead, through the proper channels, to the formation of new Committees by the British Association. He stated that his own Society (Hertfordshire) had already rendered material assistance to the Erratic Blocks Committee of the Association, and they hoped to render similar service to the Underground Waters Committee.

The following resolution, framed with the object of keeping the Corresponding Societies informed of the work being done by the British Association Committees, was moved by Dr. Garson, seconded by Captain Galton, and passed unanimously:—

"That the Secretary of the British Association be requested to send a list of the several Committees appointed by the Association to each of the Delegates of the Corresponding Societies, or to the Secretaries of these Societies, as soon as possible after the meeting of the Association, together with a copy of the proceedings of the meetings of the Conference of Delegates."

At the second Conference the chair was taken in the absence of Dr. Williamson by Prof. Boyd Dawkins, F.R.S., the Corresponding Societies Committee being represented by Dr. Garson, Mr. John Hopkinson, F.L.S., and the Secretary, Prof. R. Meldola, F.R.S.

The Secretary read the minutes of the proceedings of the first Conference, and it was stated that in accordance with the decision then arrived at the Delegates had been placed on the respective Sectional Committees as "Delegate Members."

The Chairman directed attention to the kind of work which might be done at the Conferences, stating that as a member of the Council of the British Association he knew that the Association was anxious to consolidate the work of the local Societies. He suggested that the best mode of procedure would be to take the different Sections *seriatim*, and hear the recommendations forwarded by the Committees of these Sections, together with suggestions by the Delegates respecting the lines of investigation in which the local Societies could take part.

SECTIONS A AND B.—No recommendations from the Committees of these Sections having been forwarded to the Secretary of the Conference, the Chairman invited suggestions from the Delegates.

Luminous Meteors.—Mr. F. T. Mott suggested that much useful work might be done if the local Societies would undertake to record systematically the appearance, position, direction, &c., of luminous meteors.

The Secretary stated that a Committee of the British Association was for many years in existence for the purpose of carrying out these observations, but, for some reason unknown to him, the Committee appeared now to have ceased its labours.

Magnetic and Tidal Observations.—Mr. J. Martin White suggested that some of the local Societies which were favourably situated for the purpose might undertake systematic observations of local tidal and magnetic phenomena.

Meteorological and Phenological Observations.—Mr. Heywood stated that many valuable meteorological observations were buried in the log-books of steamships, and suggested that some of the local Societies might render good service to meteorology by examining these books and keeping records of any important entries. Mr. Hopkinson pointed out two ways in which the local Societies might advance meteorological science. In the first place he thought that many observers in different parts of the country might be in the habit of recording the rainfall or other meteorological phenomena without communicating the results to Mr. Symons. Good service would be rendered if the Corresponding Societies would find out such observers and put them into communication with Mr. Symons.[1] In the next place he suggested that observations of the time of flowering of plants, first appearances of birds and insects, &c., might be systematically recorded and forwarded to the Royal Meteorological Society by those observers who had not hitherto been in the habit of communicating their results to that Society.

SECTION C.—Mr. C. E. De Rance, F.G.S., attended the Conference on behalf of the Committee of this Section. The three following recommendations were forwarded by the Secretary of the Section:—

Sea Coasts Erosion.—"That Messrs. R. B. Grantham, C. E. De Rance, J. B. Redman, W. Topley, W. Whitaker, and J. W. Woodall, Major-General Sir A. Clarke, Admiral Sir E. Ommanney, Sir J. N. Douglass, Captain J. Parsons, Captain W. J. L. Wharton, Prof. J. Prestwich, and Messrs. E. Easton, J. S. Valentine, and L. F. Vernon Harcourt be reappointed a Committee for the purpose of inquiring into the Rate of Erosion of the Sea Coasts of England and Wales, and the influence of the Artificial Abstraction of Shingle or other Material in that Action; that Messrs. De Rance and Topley be the Secretaries."

Underground Waters.—"That Prof. E. Hull, Dr. H. W Crosskey, Captain Douglas Galton, Prof. J. Prestwich, and Messrs. James Glaisher, E. B. Marten, G. H. Morton, James Parker, W. Pengelly, James Plant, I. Roberts, Fox-Strangways, T. S. Stooke, G. J. Symons, W. Topley, Tylden-Wright, E. Wethered, W. Whitaker, and C. E. De Rance be reappointed a Committee for the purpose of investigating the Circulation of the Underground Waters in the Permeable Formations of England, and the Quality and Quantity of the Waters supplied to various towns and districts from these formations; and that Mr. De Rance be the Secretary."

Erratic Blocks.—"That Profs. J. Prestwich, W. Boyd Dawkins, T. McK. Hughes, and T. G. Bonney, Dr. H. W.

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[1] G. J. Symons, F.R.S., 62, Camden Square, London, N.W.

Crosskey, and Messrs. C. E. De Rance, H. G. Fordham, J. E. Lee, D. Mackintosh, W. Pengelly, J. Plant, and R. H. Tiddeman be reappointed a Committee for the purpose of recording the position, height above the sea, lithological characters, size, and origin of the Erratic Blocks of England, Wales, and Ireland, reporting other matters of interest connected with the same, and taking measures for their preservation; and that Dr. Crosskey be the Secretary."

Mr. De Rance described the above three inquiries undertaken by Section C, in which it was thought the Corresponding Societies could render valuable assistance. Forms of inquiry had been circulated largely by these Committees, and it was suggested that any work done by the Corresponding Societies should be on these forms printed by the British Association. Mr. De Rance stated that forms would always be supplied to the Secretaries of Corresponding Societies applying for them.

Dr. Crosskey made some remarks explanatory of the work of the Erratic Blocks Committee. He stated that the assistance of the local Societies would be particularly valuable in this inquiry, and that he would be happy to supply the necessary forms to the Corresponding Societies in the hope that they would be filled up. He urged upon the Delegates the necessity for preserving these boulders, which were everywhere being broken up, and were rapidly disappearing from off the face of the country.[1]

Earth-Tremors.—Prof. Lebour stated that for some time past the North of England Institute of Mining and Mechanical Engineers had had a Committee actively engaged on the subject of earth-tremors and their possible connection with mine-explosions. This subject was naturally related to those of Sections A, C, and G of the British Association, and its investigation might be powerfully promoted by them. Some of the Corresponding Societies might aid greatly in making and recording observations on earth-tremors in various parts of the country. The more extensive the area over which such observations were made (if by competent observers and with suitable instruments) the more valuable they become; but it was very important that there should be some general understanding between the observers in different parts of the country, in order that some degree of that uniformity which is so desirable in matters of this kind should be attained. The cost of the expensive instruments necessary would be much lessened if large numbers of them were used. The question of earth-tremor observations was only one of many in which the engineering Societies and the British Association could be mutually useful, the former carrying out the work and the latter lending the influence of its official recognition and support.

The Rev. J. M. Mello stated that colliery proprietors were generally unwilling to spend money in investigations unless some very specific form of inquiry was circulated.

Mr. Hopkinson remarked that the Corresponding Societies, if supplied with the necessary forms, would no doubt be willing to circulate them among their members. Mr. Heywood thought the suggestion for observing and recording earth-tremors a most valuable one, and he remarked that the Cardiff Society would be happy to assist in the investigation if the formation of a Committee was sanctioned by the Association.

SECTION D.—The Committee of this Section was represented by Prof. W. Hillhouse, M.A., F.L.S.

Preservation of Native Plants.—In reply to a question by the Secretary, Prof. Hillhouse stated that in response to the inquiries which he had circulated among the Delegates and others likely to furnish information, he had received details from twelve or fourteen localities recording between two and three hundred disappearances of plants. Mr. Stirrup stated that for years past a great destruction of plants had been going on in the Manchester district, and the local Societies had found it necessary to strongly inculcate among their members the necessity of preventing this extermination. Mr. Hopkinson remarked that a similar rule had been always observed by the Hertfordshire Society with respect both to animals and plants, and he thought that all the local Societies should adopt it. Mr. Mott pointed out that one practical result illustrating the benefit of Prof. Hillhouse's resolution had been the omission of the localities of all the rare ferns and orchids from the flora of Leicestershire, which his Society was just about to publish.

Local Museums Committee.—Mr. Mott stated that a joint Committee, composed of representatives of Sections C and D, had been recommended for appointment for the purpose of reporting upon the provincial museums of the United Kingdom. The work of this Committee would be much facilitated by the co-operation of the local Societies, and he hoped that the Delegates would bring the matter under the notice of their respective Societies. The Committee consists of Mr. V. Ball, Mr. H. G. Fordham, Profs. Haddon and Hillhouse, Dr. Macfarlane, Prof. Milnes Marshall, Mr. Mott (Secretary), Dr. Traquair, and Dr. Henry Woodward.

In reply to a question as to whether the work of this Committee was to be confined to public or to extend to private museums, Mr. Mott stated that it might be found desirable to extend the report to some few private museums.

The Chairman remarked that the Local Museums Committee was one of the most important that had yet been formed. The local museums of this country were generally in a most deplorable state, and one of the first things to be done was to exclude from such collections all extraneous specimens that were not truly local. According to his experience, he had found that it was impossible for a local Society to flourish and at the same time to carry on a large museum successfully. The two organisations should be independent, but at the same time it was most desirable that the objects collected by local Societies should be handed over to the nearest local museum. With reference to this question of local museums, he considered that we in this country were much behind Germany, America, and France.

A short discussion took place with reference to the naming of specimens in local museums, in which Mr. Eve, Mr. Hopkinson, and the Chairman took part.

SECTION H.—The Committee of this Section was represented by Dr. Garson, who stated that one Committee which was about to be formed on the recommendation of their Section had arisen from the suggestion made by Mr. J. W. Davis at the last Conference.

Prehistoric Remains.—The following is the resolution sent up to and adopted by the Committee of Recommendations:—"That Sir John Lubbock, Dr. R. Munro, Mr. Pengelly, Prof. Boyd Dawkins, Dr. Muirhead, and Mr. J. W. Davis be appointed a Committee to ascertain and record the localities in the British Islands in which evidence of the existence of prehistoric inhabitants of the country is found."

Prof. Meldola stated that three years ago he had brought this subject under the notice of the Delegates in a paper which he had read at the Southport meeting of the Association, and which had been published in abstract in the volume of Reports for 1883, and *in extenso* in the *Transactions* of the Essex Field Club.[1] He remarked that the work which the Committee proposed to undertake was of the greatest national importance in view of the great destruction of ancient remains that had been going on for many years.

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[1] The addresses of the Secretaries of these three Committees are:—
Underground Waters.—C. E. De Rance, F.G.S., A.I.C.E., 28, Jermyn Street, London, S.W.
Erratic Blocks.—Rev. H. W. Crosskey, LL.D., F.G.S., 117, Gough Road, Edgbaston, Birmingham.
Sea Coasts Erosion.—Wm. Topley, F.G.S., A.I.C.E., 28, Jermyn Street, London, S.W.

[1] See NATURE, vol. xxix. p. 19.

The Chairman remarked that the subject was undoubtedly one of great importance, and some of the local Societies had already commenced to record the position of these remains on the Ordnance maps. He stated that according to his experience the 1-inch map could be used, but the 6-inch map would be found much better. One desideratum in the work was a good system of symbols; such a system had been employed in a map of ancient remains recently published in France, and he stated that he should be happy to place this system at the disposal of the Committee. He added that he was glad to be able to announce that he had succeeded in getting an Act passed for the preservation of the ancient monuments of the Isle of Man.

Preservation of Stonehenge.—Dr. Garson stated that the Committee of Section H had forwarded a resolution to the Committee of Recommendations with reference to the preservation of Stonehenge, and, pending its consideration by this Committee, it had been suggested that it should also be brought under the notice of the Corresponding Societies through their Delegates, with the object of these using their influence, as far as possible, for the preservation of this and other monuments throughout the country. The following is the resolution referred to :[1]—"That the attention of the proprietor of Stonehenge be called to the danger in which several of the stones are at the present time from the burrowing of rabbits, and also to the desirability of removing the wooden props which support the horizontal stones of one of the trilithons, and, in view of the great value of Stonehenge as an ancient monument, to express the hope of the Association that some steps will be taken to remedy these sources of danger to the stones."

This resolution had originated last April during a joint meeting of the Geologists' Association and the Hampshire Field Club on Salisbury Plain, when copies were ordered to be forwarded to the proprietor, to the Inspector of Ancient Monuments, and to the Secretary of the Corresponding Societies Committee of the British Association. The proprietor of these valuable remains had hitherto refused to take advantage of the Ancient Monuments Act, though repeatedly requested to do so, neither had he paid due attention to their proper preservation, so that it had been thought desirable to move the foregoing resolution which had been sent to the proper quarter for confirmation by the General Committee of the Association.

Election of Corresponding Societies.—At the termination of the Conference, Mr. Davis raised the question whether a Corresponding Society when once admitted by the Association should not always be retained on the list.

The Secretary explained that the election of Corresponding Societies took place *annually*, and that each of these Societies would be expected to make an annual application for re-election on the printed forms sent out before June. There was no reason why a Society when once elected should not be re-elected every year as long as it kept up its scientific activity. He was of opinion that a failure on the part of a Corresponding Society to send a Delegate to any meeting of the Association should not disqualify that Society for re-election, although it was expected that when a Delegate did attend the meeting of the Association he should be present at the Conferences. Prof. Meldola further stated that some few of the Societies which had been elected last year did not appear in this year's list, the reason being that the Secretaries had not filled in and returned the printed forms sent out at the beginning of the year, nor had any notice been taken of a second application asking whether it was the wish of their Society to be re-elected; so that, as Secretary of the Corresponding Societies Committee, he had concluded that these Societies desired to withdraw, and they had accordingly been removed from the list.

THE COLONIAL AND INDIAN EXHIBITION

RESUMING our notes on some of the principal exhibits (NATURE, vol. xxxiv. p. 548), the Court running parallel with and between those of Mauritius and Seychelles on the one side and Cyprus and Malta on the other was that which contained the collection from the

WEST INDIES.

Vegetable products, as might be expected, formed the bulk of the exhibits in this attractive Court, which had an air of comfort and finish not excelled in any other part of the building. Entering the Court from the northern end, the first bay on the left hand was devoted to

Trinidad, an island celebrated both for the quantity and quality of the cocoa grown upon it, which indeed is the staple article of produce. The value of cocoa exported from Trinidad in 1885 is stated in the Official Hand-book to have amounted to 421,974*l.*, and in some "Notes on Trinidad Industries," by Mr. John McCarthy, F.C.S., the Assistant Commissioner for Trinidad, recently published, it is stated that the quantity of cocoa imported into England in 1885 amounted to 10,560 tons, against 10,120 tons in 1884, and 9986 in 1881. Numerous specimens of cocoa seeds are exhibited, as well as prepared cocoa and chocolate.

Mr. McCarthy describes the cultivation of the cocoa-nut (*Cocos nucifera*) as a very profitable industry, though the tree does not bear much before it is eight years old. Experiments, he tells us, "are now being tried in Trinidad to make it act as a shade tree to the cocoa (*Theobroma*)" instead of planting the quick-growing "Bois immortelle." The idea of this planting is to realise from the same land a double crop, namely, that from the *Theobroma* and that from the *Cocos*. It is estimated that seventy trees planted upon an acre of land would, when in full bearing, yield 5000 nuts per annum, which would net, on an average, from 3*l.* to 4*l.* per thousand in Trinidad. The annual import of nuts into London is said to be about 12,000,000, besides which, New York imports enormous quantities, and they are also used to a very large extent for the expression of oil in Trinidad itself. Coffee has also a prominent place in the products of Trinidad, and the plant is stated to thrive well, although it has not yet produced even sufficient coffee for home consumption. More attention has, however, been directed of late to coffee culture in the island, so that it is largely increasing. The cultivation of tobacco is also an industry that promises to become of some importance, and the tobacco is described as being second only to the finest Havana. There is a good exhibit of cigars, which are said to have met with general favour, so that a demand has arisen for them.

Bahamas.—In the Official Hand-book, Sir Augustus Adderley gives a very readable sketch of the history of these islands, and briefly refers to the natural products, foremost amongst which are corals and sponges. He describes the "sponging and wrecking vessels" as fine models and fast sailers, built by the islanders of native hard wood known as "horseflesh," and planked with yellow pine obtained from North Carolina. Conch shells are exported in large quantities to the value of about 1200*l.* per annum, and the pale pink pearls which are found in them to the extent of 3000*l.* per annum. The sponge exports were estimated at 60,000*l.* for 1885. Mention is made of the abundance of plants valued as medicines, many of which might be further developed by systematic trial of their effects in this country. Perhaps the two best known medicinal plants are the Canella Bark (*Canella alba*, Murr.) and the Sweet Bark or Cascarilla

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[1] This resolution was adopted by the Committee of Recommendations and confirmed by the General Committee.

(*Croton Eleuteria*, J. J. Benn.). The first has a bitter, acrid, and pungent taste, and a cinnamon-like smell. With us it is used as an aromatic stimulant, and as a condiment in the West Indies. The sweet bark is a bitter aromatic tonic, formerly used as a substitute for Peruvian bark, but now chiefly as an ingredient in pastilles and for mixing with tobacco for the sake of its pleasant musky odour. The cultivation of perfume-yielding plants is recommended as a probable commercial success, the demand for perfumes at the present time being so great that it has even been proposed to cultivate in Australia on a large scale such plants as are now grown at Grasse, Nice, and Cannes.

Jamaica.—The contents of this Court were both numerous and varied. Rum and sugar were fully illustrated by a large number of samples. Coffee was also well represented; of this article the Official Catalogue states that two distinct classes are produced in the island, the total annual export being about 84,000 cwt. per annum, of which about 10,000 cwt. is Blue Mountain coffee, a fine quality, consigned almost entirely to the Liverpool market. Pimento or allspice is a product exclusively of Jamaica, where it is grown in plantations or gardens known as "pimento walks." The commercial article consists of the dried berries, which were exported from Jamaica to the value of 53,867*l.* in 1885. It is very largely used as a spice as well as in medicine, on account of its aromatic and stimulant properties. The fruits contain a quantity of oil, which is obtained by distillation, and is used in perfumery and for similar purposes to which clove-oil is put. Pimento-sticks are amongst the strongest and best for walking-sticks and umbrella-handles, on account of their strength, rigidity, and non-liability to crack. The pimento-tree is of low growth, and is known to botanists as *Pimenta officinalis*.

In this Court were shown some remarkably fine samples of Annatto seeds (*Bixa Orellana*), noted for their plumpness, as well as for their bright colour, the waxy coating of the seeds being highly valued as a red colouring-matter. A large and interesting collection of fruits preserved in a salt-solution were here shown; amongst others the following will attract attention: Star-apple (*Chrysophyllum Cainito*), Cocoa-plum (*Chrysobalanus Icaco*), Blimbing (*Averrhoa Bilimbi*), Akee (*Cupania edulis*, better known, perhaps, as *Blighia sapida*). Many of these are the produce of introduced plants, and the fruits are for the most part fine examples. Amongst a number of specimens of essential oils from well-known plants, most of which are apparently of excellent quality, are some that are but very little known, such, for instance, as those from the Bermuda Cedar (*Juniperus bermudiana*), the Mountain Cigar Bush (*Hedyosmum nutans*), Mountain Thyme (*Micromeria obovata*), Cigar Bush (*Critonea dalea*), and the Sand Box-tree (*Hura crepitans*).

Barbados.—The exhibits from this island consisted largely of similar produce to the islands already referred to. As illustrating the extent of land occupied by sugar cultivation, it is stated in the introductory notice of Barbados in the Official Hand-book, by the Hon. C. C. Knollys, that "out of a total acreage of 106,470 acres, an area of 100,000 acres is devoted to canes." Tobacco is recommended for extended cultivation, and root-crops such as arrowroot and cassava give heavy returns.

British Honduras.—We take this dependency in this order, as it occupied a position in the Exhibition next that of Barbados. The importance of timber in the produce of British Honduras is seen by a simple glance at the exhibits, and to the future development of these timber resources lies in a very great measure the future prosperity of the colony. In the introductory notes to these exhibits the following paragraph occurs:—"To its timber and dye-woods the colony of British Honduras owes its existence, and whatever measure of progress and advancement it may have attained. To the discovery, first of logwood, and subsequently of mahogany, its original settlement must be ascribed." Notwithstanding the importance of the forest produce, very few of the timbers are yet known either to commerce or to science, but many of them are of exceptional hardness and beauty. Mahogany is, of course, the most important wood in the colony, and, next to it, the cedar (*Cedrela adorata*), which is not only exported to a very large extent, but is also used in the colony for light indoor work—cigar-boxes, trunks, packing-cases, and for dug-out canoes, several of which were exhibited. Amongst a collection of lianes, or climbing-plants, is a specimen of the chew-stick (*Gouania domingenesis*), with the singular information, besides that of its use as a tooth-brush and tooth-powder, that "it is used in place of yeast to start fermentation in making ginger- and spruce-beer, &c." Probably the most striking object in this Court is a large and beautifully figured slab of mahogany; the dark wavy cross-markings are extremely beautiful and very remarkable in this wood; the plank is, moreover, without a flaw.

Dominica.—The space occupied by this island, as well as by the remaining colonies, was small; the exhibits on the whole, however, were interesting, and some were worth noting, such, for instance, as the husks or shells of the Liberian coffee, which are said to be worth from 1 to 2 cents. per pound in the United States, the fruits of *Acacia Farnesiana*, stated to be used in tanning, and bark of Guava, the *Psidium Guayava*, which is rich in tannin, and is used as an astringent. Raw lime-juice is exported from Dominica in increasing quantities, but the greater part of the juice is boiled down until ten or twelve gallons are reduced to one, and is shipped in this concentrated form to England and the United States for the manufacture of citric acid.

Montserrat.—Sugar and lime-juice are the principal staples of this island, and these were the most prominent exhibits.

St. Kitts and the Virgin Islands.—From these islands the exhibits were but small, and without special interest.

Antigua.—The chief product of this colony is sugar, the average crop of which for the last twenty years is stated to have been about 12,000 hogsheads. Yams, potatoes, and Guinea corn are also grown in large quantities for native consumption. The exhibits were for the most part such as were shown in other West Indian Courts.

Grenada.—Cocoa is the most important article grown here, and some very fine fruits of good colour were shown, as well as nutmegs (*Myristica fragrans*) custard apples, or bullock's heart (*Anona reticulata*), papaws (*Carica Papaya*), Kola nuts (*Cola acuminata*). These latter were remarkably fine specimens. A good deal of attention, we are glad to see, has recently been paid to its cultivation. The tree exists in all parts of the island, and was introduced in years past by the African slaves, who used to regard it as a specific against intoxication.

Tobago.—The productive resources of this small island are varied, and were well exemplified in the collection of fruits, seeds, fibres, &c. The collection of preserved native fruits in syrup, and jellies prepared from them, was a special feature in this Court, a sample of preserved or candied papaw (*Carica Papaya*) being especially good.

St. Lucia.—Sugar, rum, and molasses are the chief products; cocoa and logwood are also produced in quantities, though the latter is stated to be at the present time a drug in the market. Tobacco, it is stated, has been tried in one district with most satisfactory results, so that it is purposed to extend its cultivation. Neither the individual exhibits in this Court, nor in the remaining one of St. Vincent, call for any special remark. We cannot conclude our notice of the West Indian exhibits without a reference to the series of over 100 water-colour drawings, by Mrs. Blake, illustrative of the flora of the West Indian Islands.

JOHN R. JACKSON

Museum, Royal Gardens, Kew

NOTES

THE President and Council of the Royal Society have this year awarded the Copley Medal to Franz Ernst Neumann, of Königsberg (For. Mem. R.S.), for his researches in theoretical optics and electro-dynamics, and the Davy Medal to Jean Charles Galissard de Marignac, of Geneva (For. Mem. R.S.), for his researches on atomic weights. Prof. S. P. Langley was awarded the Rumford Medal for his researches on the spectrum by means of the bolometer. The Royal Medals have, with the approval of Her Majesty, been awarded to Mr. F. Galton and Prof. Guthrie Tait, the former eminent for his statistical inquiries into biological phenomena, and the latter for his various mathematical and physical researches. The medals will be presented at the anniversary meeting on November 30.

MR. CHARLES WILLIAM PEACH, the eminent scientific observer, died in February last, and, not long afterwards, a memorial was addressed to the First Lord of the Treasury, praying that his daughter, Jemima Mary, might, on account of her very slender provision, be placed on the Civil List. The memorial, subscribed by about 140 eminent persons, resulted in a Treasury grant of 200*l*. being sent to Miss Peach, after the expiry of five months. The grant so made being totally inadequate by way of provision, while it fails to denote the high sense entertained of Mr. Peach's scientific services, it has been determined to secure, by private subscription, the means of providing Miss Peach with a permanent annuity. Of the sum necessary to effect this, the Treasury grant of 200*l*. will of course form the nucleus. The Committee believe it is unnecessary to do more than allude to Mr. Peach's more conspicuous services. For half a century he gratuitously supplied to contemporary inquirers the fruits of his research. When Mr. Hugh Miller was engaged in preparing his work on the "Old Red Sandstone," Mr. Peach conveyed to him those specimens from Caithness which materially availed him in illustrating his subject. By his discovery of Silurian fossils in the rocks of Cornwall, he enabled Sir Henry de la Beche, then at the head of the Geological Survey, to obtain a scientific basis for mapping the rocks of Devon and Cornwall. In connection with this important work, also, by his discovery of Lower Silurian fossils in the north-west of Scotland—thereby affording the key by means of which the structure and age of the rocks of the Scottish Highlands must be determined—it was the opinion of Sir Roderick Murchison that Mr. Peach had rendered service such as merited a special recompense from his country. From the Devonian rocks of Cornwall and the Old Red Sandstone of the north of Scotland he procured the fish fauna which supplied a share of the material used by Sir Philip Egerton, Prof. Huxley, and Prof. E. Ray Lankester in preparing their several descriptions. In their monographs, Mr. Darwin and Dr. Carpenter have acknowledged his valuable contributions to a knowledge of the Balanidæ and the Polyzoa, while many other naturalists were also indebted to him for most important zoological observations made along our coasts. Mr. Peach made a valuable collection of the fossils of Brora, Sutherlandshire (Jurassic), now in the British Museum. His discoveries have largely availed in elucidating the fossil flora of the Old Red Sandstone and the Carboniferous rocks of Scotland. In the department of recent marine animals and plants, he has added hundreds of new species to the British lists. In acknowledgment of his scientific acquirements, he received honours from the leading scientific Societies; and in 1875 he was awarded by the Royal Society of Edinburgh one of their gold medals. After twenty-one years of arduous labour in connection with the Coastguard, Mr. Peach was in 1845 transferred by Sir Robert Peel to the Department of Customs, as suggested by the Council of the British Association; but this change, while adding to his leisure, did not materially enhance his emoluments. In the public service his highest income was 150*l*., his retiring allowance being 130*l*. Such remuneration as he received for his scientific services he applied exclusively to the cause of research. He attained his eighty-fifth year, and in his old age it was a source of deep anxiety to him as to how he should be able to provide for the devoted daughter to whose help and affectionate care he was so much indebted. Five hundred pounds are wanted, and this amount there ought not to be much difficulty in procuring. An account is opened in the Bank of Scotland, for the receipt of contributions, under the care of Mr. Robert Gray, Bank of Scotland, Edinburgh, as Treasurer of the fund. Among the members of the Committee are Sir William Turner, F.R.S.; Sir Joseph D. Hooker; Archibald Geikie, F.R.S., Director-General of the Geological Survey of Great Britain and Ireland; Prof. E. Ray Lankester, F.R.S.; Prof. Tait; John Murray, V.P.R.S.E., Director of the *Challenger* Expedition Commission, Edinburgh; William Pengelly, Torquay; and others.

OUR readers must have noticed the recent telegrams concerning the beleaguered position of Dr. Emin Bey at Wadelai, on the Upper Nile, some 50 miles north of Lake Albert Nyanza. Emin Bey was Governor of the old Equatorial Province of Egypt, and his administration of the province was of a model character. Moreover, he did much before the Mahdi insurrection broke out for the promotion of a knowledge of the natural history and geography of the Upper Nile region, as will be seen from his many communications to *Petermann's Mitteilungen*. His last communication is in the current number of the *Mitteilungen*, and is dated January last. In it, notwithstanding his critical position, he speaks of his collections. When the news of Emin Bey's position first reached this country, the Government regarded it as their duty to do what they could to rescue or succour him, and the Intelligence Branch of the War Office made all inquiries as to routes, among other things taking counsel with Mr. Joseph Thomson. There are many difficulties in the way, especially since the death of Mtesa, King of Uganda; but the Government, we believe, have by no means given up the idea of communicating with Emin Bey. Mr. Stanley has expressed his willingness to lead an expedition, and Mr. Thomson shows, in yesterday's *Times*, how the thing can be done. He believes rightly that the route across Masai Land followed by himself is the directest and shortest, and it is really not necessary to pass through Uganda at all; a sweep could be made round by Lake Baringo and the Suk country, and so westwards 300 miles to Wadelai. Moreover, it seems to us that the route by the west side of the Victoria Nyanza on to the Albert Nyanza is worthy of consideration. Certainly, if Mr. Thomson undertakes to lead a relief expedition, he could accomplish it speedily and peacefully. The English Government is bound to do everything in its power to prevent any disaster falling upon so valuable a life; and if they mean to do anything it ought to be quickly, or else it may be too late.

M. PASTEUR, according to the *Times* Paris Correspondent, exhausted by the incessant labours of the last few years, was to leave on Tuesday, by the advice of his family and friends, for Bordighera, where M. Bischoffsheim has placed his villa at his disposal. The *Times* Correspondent, before his departure, ascertained from M. Pasteur the precise state of the Pasteur Institute subscription and of his experiments. The subscription has now nearly reached 1,800,000*f*., but contributions still flow in, though rather more slowly, and M. Pasteur has reason to hope that we shall eventually reach the sum required. The Paris Municipality has given a gratuitous lease for 99 years of 2500 metres of ground, the site of the old Collège Rollin. This area being insufficient for the laboratories, not merely for rabies, but for other contagious maladies, he has asked for a lease for 99 years of 2500 metres adjoining, and he expected that this proposal would be acceded to at Monday's sitting. A subscription is being raised among the brewers in England. M. Pasteur then goes on in his communication to describe the results of his operations much in the same terms as in his paper to the Paris Academy epitomised in a recent number of NATURE.

THE following are among the lectures to be given at the London Institution, during 1886-87:—Sir R. S. Ball, F.R.S., Astronomer Royal of Ireland, two lectures on "The Astronomical Theory of the Great Ice Age," one given last Monday, the other for November 29; Prof. E. Ray Lankester, F.R.S., six lectures on "The Elements of Biology," Thursdays, November 25, December 2, 9, 16, 23, 30; Prof. T. W. Rhys Davids, Ph.D., "Buddhism," Monday, December 13; Henry Seebohm, "Birds' Nests and Eggs," Monday, December 20; Eric S. Bruce, "War and Ballooning," Monday, December 27; Dr. C. Meymott Tidy, F.C.S., three lectures (juvenile) on "Chemical Action," Thursdays, January 6, 13, 20; Prof. W. H. Flower, F.R.S., Director of the Natural History Department, British Museum, "Fins, Wings, and Hands," Monday, January 17; Prof. Silvanus Thompson, Ph.D., two lectures on "Electric Bells," Thursdays, February 10, 17; Harold B. Dixon, F.R.S., "The Lighthouse Experiments at the South Foreland," Thursday, February 24. The Thursday lectures will be given at 6 o'clock, excepting on January 27, February 3, March 3, and March 10, when they will be given at 7 o'clock. The Monday lectures are at 5 p.m.

GENERAL J. F. TENNANT sends us the following additional information on the late Major-General John Theophilus Boileau, whose death we announced last week:—"General Boileau was selected to superintend one of the magnetic observatories established by the Honourable East India Company in 1843 in connection with the general scheme of magnetic observatories, and had charge of the Simla Observatory. Long after it was closed for observing purposes he was employed in reducing and publishing the results. He also published a collection of astronomical, magnetical, and meteorological tables, and a set of traverse tables; and possibly some special tables, which, being published in India, have never come into much use, and have practically been superseded by others more recent. General Boileau has long been annually appointed one of the Scrutators at the anniversary meeting of the Royal Society on St. Andrew's Day, and we shall miss there on Tuesday a familiar face and name. His energies and time have long been absorbed in institutions for helping those in want, especially the daughters of officers of the army and soldiers. And now his, in turn, want aid, which an influential Committee is endeavouring to raise for them. Will you lend the aid of your circulation to make known the want among those who can spare?"

WITH reference to the above note we heartily commend to our readers the proposal to raise by public subscription a testimonial in recognition of the devotion displayed by General Boileau over a long period of years in philanthropic works, especially those so ably and successfully carried out by him on behalf of the Royal School for Daughters of Officers of the Army at Bath, and the Soldiers' Daughters' Home at Hampstead. An influential Committee has been formed for giving effect to the proposal, with Field-Marshal the Lord Napier of Magdala, G.C.B., G.C.S.I., R.E., as Chairman, and Major-General Philip Ravenhill, C.B., as Honorary Secretary. It is contemplated that the amount collected will best be expended in purchasing annuities for certain members of General Boileau's family, who are at his death left in very straitened circumstances. The Committee appeal not only to those who are, or have been, connected with either of the two institutions above named, but also to General Boileau's numerous friends and acquaintances, to aid them in attaining the object they have in view. Subscriptions will be received by Messrs. Cox and Co., Craig's Court, London, S.W., by the Honorary Secretary, 50, Holland Road, Kensington, W., or they may be paid to any member of the Committee.

THE *Methodist Times* announces the formation of a "Wesley Scientific Society" for the purpose of promoting intercourse among Wesleyan students of science. It will aim at the encouragement of practical scientific work among amateurs, the guidance of beginners in the study of natural history, the interchange of opinions upon scientific questions, and the collection and circulation of useful facts and observations bearing upon the sciences in general. If sufficient support is promised, the first number of a monthly journal will be issued by next March. The President is the Rev. W. H. Dallinger, F.R.S., and the Secretary is the Rev. W. Spiers, M.A., F.G.S. The Vice-Presidents are Rev. G. Bowden, Rev. N. Curnock, A. C. Graham, M.A., C. W. Kimmins, D.Sc., J. Potts, F.G.S., and Rev. G. S. Rowe. The Rev. Dr. Dallinger, Rev. W. Spiers, and Rev. Hilderic Friend, F.L.S., will edit the Society's journal.

THE great refracting telescope of the Bischoffsheim Observatory is in full operation at Nice. It is second only to the Pulkowa instrument. Observations with it have been conducted most successfully.

ADMIRAL MOUCHEZ, Director of the Paris Observatory, has issued circulars in the name of the Committee for erecting to François Arago a statue on the southern part of the meridian line which passes through that establishment. Subscriptions are received at the Observatory by M. Mouchez. A sum of about 400*l.*, which had been collected for a similar purpose when Arago died thirty-two years ago, is in the hands of the Paris Academy of Sciences, and will be placed at the disposal of the Committee.

IT is stated that a subscription will be started at Auxerre, the native place of M. Paul Bert, for erecting a memorial on his behalf.

LAST week a boat containing fourteen persons was successfully worked on the Seine with artificial wings acting on the air, and propelled by a rotating wheel.

DR. FOREL informs us that earthquakes occurred in Switzerland on the following dates:—At Cernetz, Grisons, November 6, 17h. 44m., and at 19h. 59m.; November 7, at 1h. 28m.; over Switzerland, with centre in the Lake of Lucerne, on November 16, 2h. 15m. (all Greenwich times).

WE have received from Mr. J. White, photographer, of Littlehampton, a copy of the last portrait (cabinet) taken of the late Prof. Guthrie. It is a very good one.

DR. R. MULLINEUX WALMSLEY, D.Sc., Senior Demonstrator at the Finsbury Technical College, in the department of Applied Physics and Electrical Engineering, has been appointed Principal of the Technical College about to be established at Kurrachee.

THE decline of the Indian silk industry is a subject which has lately attracted some attention. Various causes have been assigned, such as rack-renting by the Zemindars, while the existence of any specific disease among the silkworms has been strenuously denied. The question seems at last to have been settled by the investigations of a skilled entomologist, Mr. Wood Mason, Curator of the Indian Museum, who, on examining a large number of living cocoons, received from various parts of the country, found over 60 per cent. so diseased that no moths emerged, while such moths as emerged were nearly all sickly and crippled, and only 6 per cent. lived to couple and lay eggs. A further examination showed that the cells of the silk glands, and all other tissues, including even the blood, were in the last stage of disease, and literally crammed with minute corpuscles, identical with those which have been demonstrated to be the cause of the fibrine disease which, in an epidemic form, from 1849 to 1865 ravaged the silkworm nurseries of France, and reduced them to a state of ruin, but which, thanks to M. Pasteur, is now practically eradicated from Europe.

PROF. GIOVANNI LUVINI has just issued, in pamphlet form, a summary of the results of his important experiments on the electric conductibility of vapours and gases. As the readers of NATURE are already aware, these experiments have finally exploded the old theory that the moist atmosphere and other vapours are good conductors. The pamphlet, which is published in Florence, includes an historical survey of the subject, and a full account of the processes adopted by the author. Electricians are reminded that this essay, together with his previous treatise on atmospheric electricity, are merely preparatory to a comprehensive work on the phenomena connected with the aurora borealis, which is now nearly completed.

LIGHTNING-FLASHES have sometimes been observed which, starting from one point, have ended in several. Some remarkable forms of flash have been lately described by Herr Leyst, of Pawlowsk Observatory. In one case a flash went a certain distance in a north-easterly direction, then divided, the two branches forming an angle of about 75°. When these had reached about 35° from each other, they turned and united again to one line. The path of the lightning thus formed a quadrilateral figure. It was further observed that the lightning flashed back in the same path, as if there were an oscillating discharge. In another interesting flash, the path was not a crooked line but a wavy band, which was lit up four times in succession with equal brilliancy. The time between the second and third and the third and fourth flashes seemed longer than that between the first and second. The thunder which followed lasted about 80 seconds.

THE additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas* ♀) from West Africa, presented by Capt. T. W. Robinson; a Puma (*Felis concolor* ♂) from El Gran Chaco, presented by Mr. Alfred Grenfell, F.Z.S.; a Malayan Bear (*Ursus malayanus*) from Malacca, presented by Miss A. Stewart Saville; a —— Souslik (*Spermophilus* ——) from California, presented by Mr. B. F. Russell; a Gazelle (*Gazella dorcas* ♂) from Barbary, presented by Edward J. Hough; four Chukar Partridges (*Caccabis chukar*) from Persia, presented by Dr. J. Huntley; a —— Toad (*Bufo* ——) from Africa, presented by Mr. E. N. Wroughton; six Roseate Cockatoos (*Cacatua roseicapilla*), seventeen Cockateels (*Calopsitta novæ-hollandiæ*), six Swainson's Lorikeets (*Trichoglossus novæ-hollandiæ*), two Red-winged Parrakeets (*Aprosmictus erythropterus*), eight Chestnut-eared Finches (*Amadina castanotis*), two Peaceful Doves (*Geopelia tranquilla*) from Australia, a Nutmeg Bird (*Munia punctularia*), two Eastern Turtle Doves (*Turtur meena*) from India, three Magpie Tanagers (*Cissopis leveriana*), two Red-crested Cardinals (*Paroaria cucullata*), a Red Ground-Dove (*Geotrygon montana*), a Yarrell's Curassow (*Crax carunculata*), a Crested Curassow (*Crax alector*) from South-East Brazil, two Hawfinches (*Coccothraustes vulgaris*), British, deposited.

OUR ASTRONOMICAL COLUMN

THE MASS OF MERCURY.—In the *Bulletin Astronomique* for October Herr Backlund has published a new determination of the mass of Mercury deduced from the perturbations produced in the motion of Encke's comet arising from its close proximity to the planet in 1878. From the apparitions of the comet in 1871, 1875, 1878, 1881, and 1885, Herr Backlund finds the reciprocal of the mass of Mercury to be 2,668,700,—thus making the mass of the planet considerably larger than has been found by recent investigators. And Herr Backlund states that, even supposing the acceleration of the comet's mean motion to have been constant during the entire period 1871–85, it is not possible to represent satisfactorily the five apparitions of the comet during that period on the assumption that the reciprocal of the mass of Mercury is greater than 5,000,000.

THE NATAL OBSERVATORY.—Mr. Neison has issued his Report, as Superintendent of the Natal Observatory, for the year 1885. The staff of the Observatory consists of the Government Astronomer, an Astronomical Assistant, and a Meteorological Assistant. Four ladies have also been employed during the year as astronomical computers. The equatorial appears to have been but little used in 1885, all the astronomical observations recorded having been made with the 3-inch transit. The total number of observations made with this instrument was 706, including transits of stars, of the sun, of the moon's limb, of the lunar crater Murchison A, and observations of zenith stars for latitude. With regard to the latter class of observations, it is proposed to determine the latitude of the Observatory with the greatest care, as one of the primary points of the geodetic triangulation of South Africa. Forty pairs of stars have been selected for this purpose, mostly differing in zenith distance by not more than 3′ or 4′. Also, with the view of better connecting the fundamental declinations of the star catalogues of northern and southern observatories, arrangements have been made for comparing, by Talcott's method, the zenith distances of a number of southern circumpolar stars with suitably placed northern stars of corresponding zenith distance. A list of thirty-two stars has been prepared for this purpose. Mr. Neison also reports on the state of his own work on the lunar theory, which he appears to consider of an official character.

COMET FINLAY (1886 *e*).—The following ephemeris of this object is by Dr. A. Krueger (*Astr. Nachr.*, No. 2755):—

For Berlin Midnight

1886	R.A. h.	m.	s.	Decl. °	′	log *r*	log Δ	Brightness
Nov. 28	21	0	50	19	10·7 S.	9·9941	9·9142	3·0
30	21	10	58	18	20·4			
Dec. 2	21	21	12	17	27·2	9·9971	9·9060	3·1
4	21	31	30	16	31·0			
6	21	41	53	15	31·9	0·0016	9·8992	3·1
8	21	52	20	14	30·0			
10	22	2	49	13	25·6 S.	0·0074	9·8941	3·1

The brightness at date of discovery is taken as unity.

COMET BARNARD (1886 *f*).—The following ephemeris of this object for Berlin midnight is by Dr. Oppenheim (*Dun Echt Circular*, No. 130):—

1886	R.A. h.	m.	s.	Decl. °	′	log *r*	log Δ	Brightness
Nov. 27	14	34	10	16	50·4 N.	0·0029	9·8864	17·8
29	14	54	42	17	23·5			
Dec. 1	15	16	12	17	46·0	9·9879	9·8640	21·2
3	15	38	23	17	56·0			
5	16	0	54	17	51·8	9·9839	9·8448	23·6
7	16	23	24	17	33·0			
9	16	45	27	17	0·1 N.	9·9920	9·8303	24·3

The brightness at date of discovery is taken as unity.

GORE'S NOVA ORIONIS.—It seems to be clearly established that this interesting star is indeed—as was from the first suspected from the character of its spectrum—a simple variable, and not one of the class to which the title "temporary" can be fitly applied. M. Dunér, who had observed the star at intervals from last December to the end of April, found (*Astr. Nachr.*, No. 2755), on renewing his observations at the end of October and the beginning of the present month, that it had unmistakably increased in brightness in the interval, and was continuing to do so. Herr Fr. Schwab and Mr. Espin confirm this conclusion, the former having observed this star early in last July, and found it then fainter than the 12th magnitude. Its period would appear to be not far from one year; Herr Schwab gives it as one or two weeks longer than a year, and as ranging in brightness from 6m. to 12½m., whilst M. Dunér assigns a period of 359·5d. to it. It is clearly of importance that it should be carefully watched during the coming winter.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 NOVEMBER 28—DECEMBER 4

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 28

Sun rises, 7h. 42m.; souths, 11h. 48m. 10·4s.; sets, 15h. 55m.; decl. on meridian, 21° 21′ S.: Sidereal Time at Sunset, 20h. 25m.

Moon (three days after New) rises, 9h. 52m.; souths, 14h. 15m.; sets, 18h. 39m.; decl. on meridian, 19° 7′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	8 40	12 35	16 30	23 11 S.
Venus	7 33	11 43	15 53	20 51 S.
Mars	10 27	14 14	18 1	24 15 S.
Jupiter	3 55	9 12	14 29	9 14 S.
Saturn	19 5*	3 7	11 9	21 25 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Star by the Moon (visible at Greenwich)

Dec.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
3	h^1 Aquarii	5½	17 8	18 30	116 286

Dec.	h.	
3	5	Venus in superior conjunction with the Sun.
3	12	Mercury in inferior conjunction with the Sun.
4	4	Mercury at least distance from the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Nov. 28,	2 27	*m*
			Dec. 3,	2 6	*m*
Algol	3 0·8	40 31 N.	,, 1,	5 38	*m*
			,, 4,	2 27	*m*
ζ Geminorum	6 57·4	20 44 N.	Nov. 29,	0 0	*m*
			Dec. 4,	0 0	*M*
U Monocerotis	7 25·4	9 32 S.	Nov. 28,		*m*
S Cancri	8 37·4	19 27 N.	,, 28,	3 9	*m*
T Ursæ Majoris	12 31·2	60 7 N.	,, 29,		*M*
S Virginis	13 27·1	6 37 S.	,, 30,		*M*
β Lyræ	18 45·9	33 14 N.	,, 28,	21 30	*m*
			Dec. 2,	5 0	*m*
R Lyræ	18 51·9	43 48 N.	Nov. 28,		*M*
η Aquilæ	19 46·7	0 43 N.	Dec. 1,	2 30	*M*
δ Cephei	22 24·9	57 50 N.	Nov. 30,	2 0	*m*

M signifies maximum; *m* minimum.

Meteor Showers

The chief shower of the week is that of the *Taurids;* R A. 60°, Decl. 49° N. Other radiants active at this time are as follows:—Near η Persei, R.A. 44°, Decl. 56° N., slow, faint meteors; near α Canum Venaticorum, R.A. 194°, Decl. 43° N., very swift, streak-leaving meteors; from Leo Minor, R.A. 155°, Decl. 36° N.; from near η Ursæ Majoris, R.A. 208°, Decl. 43° N. Fireball dates, November 29 and December 2.

TEN YEARS' PROGRESS IN ASTRONOMY[1]

II.

The Solar Spectrum.—In 1877 Dr. Henry Draper, of New York, by a series of most laborious, time-consuming, and expensive researches, discovered the presence of oxygen in the sun, evidenced in his photographs, not by fine dark lines, as in the case of elements previously recognised, but by bright, hazy bands. It is difficult to assign any reason why this gas should behave so peculiarly and so differently from others, and for this reason many high authorities are indisposed to accept the discovery. But the evidence of the photographs seems fairly to outweigh any such purely negative theoretical objections.

Other advances have been made in the study of the spectrum, due mainly to the great improvements in spectroscopic apparatus. Until recently it has not been easy to decide with certainty as to some lines in the spectrum whether they were of solar or telluric origin; the great bands known as A and B, for instance. It was only in 1883 that the Russian, Egoroff, succeeded in proving that these are produced by the oxygen in the earth's atmosphere. In his experiments, on a scale previously unknown, the light was transmitted through tubes more than 60 feet in length, closed at the end with transparent plates, and filled with condensed gas.

It was quite early pointed out that the sun's rotation ought to produce a shift in the position of lines in the spectrum according as the light is derived from the advancing or receding edge of the solar disk, and Zollner thought he could perceive it. The earliest *measures,* however, were, I believe, those obtained independently by Vogel and the writer in 1876. In the great bisulphide of carbon spectroscope of Thollon the displacement becomes easy of observation; and very recently Cornu, by taking advantage of it, and by an extremely ingenious arrangement for making a small image of the sun to oscillate across the spectroscope slit two or three times a second, has been able to discriminate at a glance between the telluric and solar lines; the former stand firm and fast, while the latter seem to wave back and forth.

In this connection also should be mentioned the great map of the solar spectrum, for which Thollon received the Lalande Prize of the French Academy of Sciences last January, and the still more accurate and important map photographed by Prof. Rowland, by means of his wonderful diffraction-gratings, and now in course of publication. Nor would it be just either to omit the earlier and less accurate maps of Fievez and Vogel, which, when published, were as far in advance of anything before them as they are behind the new ones; nor the maps just made by Prof. Smyth, of Edinburgh.

It was in connection with the construction of such a map by Mr. Lockyer, that he was led to his theory of the compound nature of the so-called chemical elements, partly as a result of his comparisons of the spectra of different substances with the solar spectrum, and partly in consequence of considerations drawn from certain phenomena observed in the solar and stellar spectra themselves. His first paper on the subject was read late in 1878. This "working hypothesis," as its author calls it, has met with much discussion, favourable and unfavourable. It unquestionably removes many difficulties and explains many puzzling phenomena; at the same time there are very serious objections to it, and some of the arguments upon which Mr. Lockyer originally laid much stress have turned out unsound. For instance, he made a great point of the fact that, after all precautions are taken to remove impurities, several elementary substances show in their spectra common lines—"basic lines" he called them—indicating, as he thought, a common component. He found in the solar spectrum about seventy of these "basic lines." Now, under the high dispersion of our newer spectroscopes, these lines, which were single to his instruments, almost without exception dissolve into pairs and triplets, and withdraw their support from his theory.

I suppose that at present the weight of scientific opinion is against him; but, for one, I do not believe his battle is lost. In view of the law of Dulong and Petit, which establishes a relation between the atomic weight and specific heat of bodies, it seems to be pretty certain that *hydrogen* cannot be the elementary "*urstoff*" out of which all other elements are made by building up, as he at first seemed disposed to maintain; this element stands apparently on no different footing from the rest. But I see no reason why the elements, as we know them, may not constitute one *class* of bodies by themselves, all built up out of some as yet more elemental substance or substances. The "periodic law" of Mendelejeff suggests such a relation. And our received theories so stumble, hesitate, and falter in their account of many of the simplest phenomena of the solar and stellar atmospheres, that a strong presumption still remains in favour of the new hypothesis. I am not prepared to accept it yet; but certainly not to reject it.

The Chromosphere.—The study of the chromosphere and prominences has been kept up, very systematically and statistically, by Tacchini in Italy, and with less continuity, but still assiduously, by several other observers. I do not know, however, that any new results of much importance have been arrived at. The list of bright lines visible in their spectra has been a good deal enlarged; and Trouvelot thinks he has observed *dark* prominences—objective forms that show, black but active, upon the background of bright scarlet hydrogen in the surrounding chromospheric clouds. It may be that he is right; but, so far as I can learn, no other observer of the solar atmosphere has seen anything similar. I certainly have not myself. And I think some of his published observations of velocities of two or three thousand miles a second in the motions of the prominences, as evidenced by the displacement of lines in the spectrum, are still more questionable.

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[1] "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young. Read May 17, 1886, before the New York Academy of Sciences. Continued from p. 69.

In two or three cases, prominences have been observed since 1876 considerably higher than any known previously. In October 1878 I myself observed one which attained an elevation of nearly 400,000 miles (13½′).

Eclipses and the Corona.—The sun's corona has been, perhaps, more earnestly studied than anything else about the central luminary, especially during the four eclipses which have occurred since 1876. At the eclipse of 1878, in the midst of an epoch of sunspot quiescence, the corona was found less brilliant than ordinary, and especially deficient in the unknown gas that produces the so-called 1474 line—the line which characterises the spectrum of the corona, and first demonstrated conclusively its solar origin in 1869. But while the corona at this time was less brilliant than it had been formerly, it was far more extensive. At least it seemed so; for, at Pike's Peak and Creston, Langley and Newcomb were able to follow its streamers to a distance of 6° from the sun. It is possible, however, that this extension was only due to the superior transparency of the mountain air.

The Egyptian eclipse of 1882 gave us some interesting results respecting the spectrum of the prominences and the corona. It appears that the light of the corona is especially rich in the ultra-violet, and in the photographs of the spectrum a number of bands are found which have been interpreted, with questionable correctness I think, as indicating the presence of carbon. The eclipse of 1883 was observed in the Pacific Ocean by French and American parties, but, I think, added very little real information. Prof. Hastings made an observation which he believed to establish a peculiar theory proposed by himself, viz. that the corona is merely a diffraction effect produced by the moon's limb, and depending on the non-continuity of phase in long stretches of light-vibrations. With a peculiar apparatus prepared expressly for the purpose, he found that at any moment the 1474 line was visible to a much greater distance from the sun on the side least deeply covered by the moon than on the other: as unquestionably would happen if his theory were correct. But the same thing would result from the mere diffusion of light by the air; and, notwithstanding his protests, the French observers who were at the same place, and nearly all others who have discussed the observations, think that this was the true explanation of what he saw. So far as I know, the discussion of the subject which has resulted from his publication has only strengthened the older view—that the corona is a true solar appendage; an intensely luminous but excessively attenuated cloud of mingled gas and fog and dust surrounding the sun, formed and shaped by solar forces.

The diffraction theory has one advantage—that it relieves us from stretching our conceptions as to the possible attenuation of matter to the extent necessary in order to account for the fact that a comet, itself mostly a mere airy nothing, experiences no perceptible retardation in passing through the coronal regions. There can be no question that this has happened several times: the last instance having been the great comet of 1882. But on careful consideration it will be found, I think, that our conceptions will bear the stretching without involving the least absurdity; a single molecule to the cubic foot would answer every necessary condition of the luminous phenomenon observed. And all the rifts and streamers, and all the radiating structure and curved details of form, cry out against the diffraction hypothesis.

The observations of the eclipse of 1885 (observed only by a few amateurs in New Zealand) have not proved important.

At present the most interesting debate upon the subject centres around the attempt of Mr. Huggins (first in 1883) to obtain photographs of the corona in full sunlight. He succeeded in getting a number of plates showing around the sun certain faint and elusive halo forms which certainly look very coronal. Plans were made and have been carried out, for using a similar apparatus on the Riffelberg, in Switzerland, and at the Cape of Good Hope. But so far nothing has been obtained much in advance of Mr. Huggins's own first results. Since September 1883, until very recently, the air has been full, as every one knows, of a fine haze, probably dust and vapour from Krakatão, which has greatly interfered with all such operations. It is now fast clearing away, and I for one am somewhat sanguine that a much greater success will be reached next winter at the Cape, and perhaps even in England during the coming summer.

Just about the same time that Huggins was photographing in England, Prof. Wright was experimenting in New Haven in a different way: isolating the blue and ultra-violet rays by the use of coloured media, stopping out the sun's disk, and receiving the image of the coronal regions on a fluorescent screen. He also had obtained what he believed, and still believes, to be a real image of the corona, when the aërial haze intervened to put an end to all such operations; for of course it is evident that whether one operates by this method or by photography, success is possible only under conditions of unusual atmospheric transparency and purity.

I suppose at present the predominant feeling among astronomers is that the case is hopeless, and that Huggins and Wright are mistaken. It may be so. But my own impression is that they are probably correct; although, of course, the matter is still in doubt.

Inferior Planets.—Leaving now the sun, and passing to the planetary system, we come first to the subject of intra-Mercurial planets.

The general opinion among astronomers (in which I fully concur) is that the question has been now fairly decided in the negative, *i.e.* it is practically certain that within the orbit of Mercury there is no planet of a diameter as large as five hundred miles, probably not one hundred. If such a one existed, it could not have failed to be discovered by the wide-angled photographs taken at the eclipses of 1882 and 1883, to say nothing of the visual observations. Of course, it is well known that at the eclipse of 1878 Prof. Watson supposed he had discovered two such bodies, and his extensive experience and his high authority led, for a time, to a pretty general acceptance of his conclusion. I notice that Dr. Ball, even very lately, in his "Story of the Heavens," is still disposed to credit the discovery. But Dr. Peters, by a masterly discussion of the circumstances of the observations themselves, and a comparison with the star maps, has shown that it is almost certain that Watson really saw only the two stars θ and ζ Cancri. In the same paper also, Peters examined all the observations of small, dark spots crossing the sun's disk which, up to that date (1879), had been made by Leverrier and others the ground for their belief in "Vulcan"; and he shows that they really afford no sufficient ground for the conclusion. As to Mr. Swift's supposed observation of two objects with large disks "both pointing to the sun," they certainly were not the two seen by Watson, while they were in the region covered by Watson and several other observers. What the precise nature of the mistake or illusion may have been it is perhaps not now possible to discover, but I think no one, unless perhaps Mr. Swift himself, now considers the observation important.

While, however, the question of a "Vulcan" is now pretty definitely settled, it is not at all impossible, or even improbable, that there may be intra-Mercurial asteroids, and that some of them may be picked up as little stars of the sixth magnitude or smaller, by the photographers at the eclipse of next August, or in 1887. The sensitiveness of our present photographic plate is now many times greater than it was even in 1882.

As to the planet Mercury, there is very little to report. It "transited" the sun in May 1878, and again in November 1881, and during the transits numerous measures were made of its diameter, giving results substantially in accord with the older values. I have already alluded, in connection with the earth's rotation, to Newcomb's investigation of former transits of this planet as establishing the sensible uniformity of the earth's rotation.

The planet Venus, by her transit in 1882, has attracted much attention, and much interest is felt as to the final outcome of the whole enormous mass of data, photographic and visual. Just how long we shall have to wait for the publication seems still uncertain. I have already said, however, that probably these transits will never again be considered as important as hitherto.

The most important physical observations upon the planet during the decade seem to be those of Langley, who, during the transit of 1882, observed a peculiar, and so far unexplained, illumination of one point on the edge of the planet's disk, and those of Trouvelot and Denning, who have observed and figured certain surface-markings of the planet. I think I may fairly mention also our Princeton observation of the spectrum of the planet's atmosphere during the transit, and our confirmation of Gruithuisen's old observation of a white cap (likely enough an ice-cap), at the edge of the planet's disk—probably marking the planet's pole, and showing that the planet's equator has no such anomalous inclination of 50° or 60°, as stated in some of the current text-books. This cap has also been observed by Trouvelot and Denning. But this lovely planet is most refractory and unsatisfactory as a telescopic object, apparently enveloped in dense clouds which mostly hide the real surface of the globe, and mock us with a meaningless glare.

We mention in passing, but without indorsement, the speculations of Houzeau, who has attempted to account for some of the older observations of a satellite to Venus, by supposing another smaller sister planet, "Neith," circling around the sun in an orbit a little larger than that of Venus, and from time to time coming into conjunction with it. But the theory is certainly untenable; a planet large enough to show phases, as the hypothetical satellite is said to have done, in the feeble telescopes with which many of the observations were made 100 years ago or more, would be easily visible to the *naked eye even*. There can be little doubt that all the Venus satellites so far observed are simply *ghosts* due to reflections between the lenses of the telescope, or between the cornea of the eye and the eye lens.

Mars.—But while Venus has gained no moons during the past ten years, Mars has acquired two, and they are both native Americans. There is no need to recount the faithful work of Prof. Hall with the then new great telescope at Washington, and its brilliant result; brilliant in a scientific sense, that is, for, regarded as luminaries, it must be admitted that the Martial satellites, in spite of their formidable names of Phobos and Deimos, do not amount to much. Under the best of circumstances, they are too faint to be seen by any but keen eyes at the end of great telescopes. Small as they are, however, the little creatures punctually pursue the orbits which Hall has computed for them, and, when the planet came to its opposition a few weeks ago, they were found just in their predicted places. They are interesting, too, from the light they throw upon the genesis and evolution of the planetary system, almost compelling the belief that they have come *gradually* into their present relation to the planet. The inner one, Phobos, revolves around the primary in 7h. 39m., which is less than one-third of the planet's day. The theory of "tidal evolution," proposed by Prof. G. H. Darwin in 1878-80, as the result of his investigations upon the necessary mechanical consequence of the tidal reactions between the earth, sun, and moon, will account for Phobos, and I know nothing else that will; though, of course, it would be rash to assert that no other account can ever be given.

Much attention has also been paid to the study of the planet's surface. In 1876 we were already in possession of three elaborate maps, by Proctor, Kaiser, and Terby, agreeing in the main as to all the characteristic formations. In 1877, Schiaparelli, of Milan, detected, or thought he did, on the planet's surface, a numerous system of "canals"—long, straight channels, some of them more than 1000 miles in length, with a pretty uniform width of fifty or sixty miles; and from his observations he constructed a new map, differing from the older ones somewhat seriously, though still accordant in the most essential features. His nomenclature of the seas and continents, derived from ancient geography, is certainly a great improvement on that of his predecessors, who had affixed to them the names of their friends and acquaintances among living astronomers. There has been some scepticism as to the reality of these "canals"; but in 1879 and 1881 they were all recovered by Schiaparelli, and several other observers, notably Burton, also made them out. Moreover, Terby finds, from drawings in his possession, that they had before been seen, though not understood or clearly recognised, by Dawes, Secchi, and other observers. At present the balance of evidence is certainly in their favour, especially as the observers at Nice report seeing them last spring. I do not think the same can be said in respect to another observation of Schiaparelli's on the same object, made in 1881. He then found nearly all of these canals—more than twenty of them—to be *double*, *i.e.* in place of a single canal there were two—parallel, and 200 or 300 miles apart. No one else so far has confirmed this "gemination" of the canals; but the planet does not come to a really favourable opposition again until 1890 and 1892, when probably the question can be settled.

The time of rotation has during the past year been determined with great accuracy by Bakhuyzen, who has corrected some errors of Kaiser and Proctor, and finds it 24h. 37m. 22·66s. In 1876 there still remained some question as to the amount by which the planet is flattened at the poles. The majority of observers had found a difference between equatorial and polar diameters amounting to between 1/100 and 1/30, while, on the other hand, a few of the best observers had found it insensible. The writer, in 1879, made a very careful determination, and found it 1/219, a quantity closely agreeing with the theoretical value deduced by Adams as probable from the motion of the newly-discovered satellites.

The Asteroids.—On May 1, 1876, the number of known asteroids was 163. To-day it stands at 258, 95 of these little bodies having been discovered within the decade, 45 of them by one man, Palisa, of Vienna, while our own Peters is responsible for 20.

None of the new ones are especially remarkable, *i.e.* some of the older ones are always more so; the most inclined and most eccentric orbits, the longest and the shortest periods, none of them belong to any of the late discoveries. One point is noteworthy, that the more recently discovered bodies are much smaller than the earlier ones. The first 25, discovered between May 1876 and October 1878, have an average opposition magnitude of 11·2, while the last 25, discovered since April 1883, average only 12·2; *i.e.* the first 25 average about 2½ times as bright as the last. Out of the whole 95, two are of the 9th magnitude (one of them, No. 234, was discovered as recently as August 1883), 14 are of the 10th, 33 of the 11th, 33 of the 12th, and 13 of the 13th. Of these last 13, 10 have been found within the past two years; and of the 12 others found in the same time, 6 are of the 11th magnitude, and 6 of the 12th.

It is clear that there can remain very few to be discovered as large as the 10th magnitude, but there may be an indefinite number of the smaller sizes.

The Major Planets.—As regards the planet Jupiter, the one interesting feature for the past ten years has been "the great red spot." This is an oval spot, some 30,000 miles in length, by 6000 or 7000 in width, which first attracted attention in 1878. At first, and for three years, it was very conspicuous, but in 1882 it became rather faint, though still remaining otherwise pretty much unchanged. In 1885 it was partly covered with a central whitish cloud, which threatened to obscure it entirely; but this season the veiling cloud has diminished, and the marking is again as plain as it was in 1882 or 1883. How long it will continue, no one can say; nor is there any general and authoritative agreement among astronomers as to its nature and cause.

In connection with observations upon this object, several new determinations have been made of the planet's rotation-period, and they all show that, as in the case of the sun, the equatorial markings complete the circuit more rapidly than in higher latitudes; a white spot near the equator gives 9h. 50m. 6s., as against 9h. 55m. 36s. for the red spot, which is approximately in latitude 30°.

We must not omit to mention Prof. Pickering's new photometric method of observing the eclipses of this planet's satellites. Instead of contenting himself with observing merely the moments of their disappearance and reappearance—an observation not susceptible of much accuracy—he makes a series of rapid comparisons between the brightness of the waning or waxing point of light during the two or three minutes of its change, using, as the standard, one of the neighbouring uneclipsed satellites. From these comparisons he determines the moment when the satellite under eclipse has just half its normal brightness, and this with a probable error hardly exceeding a single second, while the old-fashioned method gave results doubtful by not less than a quarter of a minute. Cornu and Obrecht have independently introduced the same method at Paris. When we have a complete twelve years' series of such observations, they will give an exceedingly precise determination of the time required by light to traverse the earth's orbit, and so, indirectly, of the solar parallax.

As regards Saturn, there is nothing to report so startling as Jupiter's red spot. A white spot, which appeared in 1877, enabled Hall to make a new determination of the rotation-period, which came out 10h. 14m. 14s. This is in substantial accord with an earlier determination of W. Herschel's (10h. 16m. 7s.), but involves a serious correction of the value 10h. 29m. 17s. given in most of the text-books. The error probably came from a servile copying of a slip of the pen made by some book-compiler, fifty years ago or more, in accidentally writing Herschel's value of the rotation of the inner ring, instead of that of the planet.

Much time has been spent in observations of the rings, and Trouvelot has reported a number of remarkable phenomena, most of which, however, he alone has seen as yet. The most recent micrometric measures have failed to confirm Struve's suspicion that the rings are contracting on the planet. Extensive series of observations have been made upon the satellites by H. Struve, Meyer, and others in Europe, and by Hall in this country. Hall's observations are especially valuable, and the series is now so nearly completed that we may soon hope to have most accurate tables. In the case of Hyperion, there is found a singular instance of a *retrograde* motion of the line of apsides of the orbit, produced by the action of an *outside* body, the effect being due to the near commensurability of the periods of Hyperion and Titan. This most peculiar and paradoxical disturbance first showed itself as an observed fact in Hall's observations; and, soon after, Newcomb gave the mathematical explanation and development. He finds the mass of Titan to be about 1/12,500 that of Saturn. It may be noted, too, that Hall's observations of the motions of Mimas and Enceladus indicate for the rings a mass less than 1/10 that deduced by Bessel: instead of being 1/100 as large as the planet, they cannot be more than 1/1000, and are probably less than 1/10,000.

The satellites of Uranus have also been assiduously observed at Washington, so that at present the Uranian system is probably as accurately determined as the Jovian, perhaps more so. The form of the planet has been shown to be decidedly elliptical (about 1/14) by observations of Schiaparelli and at Princeton; and the same observers have detected faint belts upon the disk, which have also been seen at Nice, and by the Henrys in Paris. Many of the observations appear to indicate a very paradoxical fact—that the belts, and consequently the planet's equator, are inclined to the orbits of the satellites at a considerable angle. The mathematical investigations of Tisserand appear to demonstrate that, in the case of a planet perceptibly flattened at the poles, satellites near enough to be free from much solar disturbance must revolve nearly in the plane of the equator; while those more remote, and disturbed more by the sun than by the protuberant equator of the planet, must revolve nearly in the plane of the planet's orbit. Thus the two satellites of Mars, the four satellites of Jupiter, and the seven inner satellites of Saturn, all move nearly in the equatorial plane, while our moon and Japetus move in ecliptical orbits. It is very difficult to believe that the satellites of Uranus, which are certainly not ecliptical and are very near the planet, do not move equatorially. And yet it is unquestionable that most of the observations with sufficiently powerful telescopes (my own among them) do seem to indicate pretty decidedly that the planet's equator is inclined as much as 15° or 20° to the orbit plane of the satellites.

As to Neptune, there is nothing new. One or two old observations of the planet have turned up in the revision of old star catalogues, and Hall, of Washington, has made a careful and accurate determination of the orbit of its one satellite, and of the planet's mass; while Maxwell Hall, of Jamaica, has deduced a very doubtful value of the planet's rotation from certain photometric observations of its brightness.

There has been some hope that a planet beyond Neptune might be found. Guided by certain slight indications of systematic disturbances in the motion of Neptune, Todd made an extended search for it in 1877-78, using the Washington telescope, and hoping to detect it by its disk, but without results. If such a planet exists, it is likely to appear as a star between the 11th and 13th magnitude, and may be picked up any time by the asteroid-hunters. But its slow motion, and the fact that our present charts give but few stars below the 11½ magnitude, will render the recognition difficult.

The indications I have spoken of, and certain others first noted in 1880 by Prof. G. Forbes, and depending upon the behaviour of certain periodic comets, furnish pretty strong reasons for believing in its existence, though as yet they fall far short of making it certain.

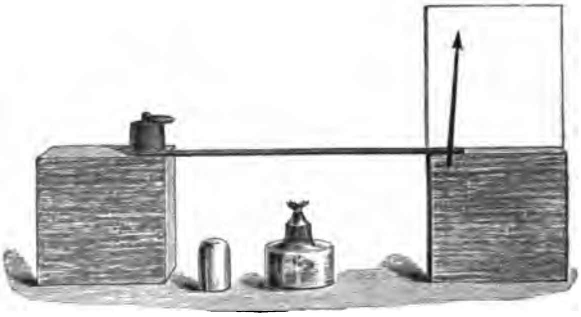
(*To be continued.*)

A LECTURE EXPERIMENT ON THE EXPANSION OF SOLIDS BY HEAT

I VENTURE to call attention to a simple and effective way of demonstrating the linear expansion of solids when heated, first suggested, I believe, by M. Kapoustine (*Journal de Physique*, December 1883, p. 576). It answers at least as well as the system of levers known as "Ferguson's pyrometer," which is usually employed for the purpose, while the cost of the apparatus is almost nothing, and any one can make it in ten minutes.

The principle is, to magnify the slight extension of a bar by causing the end of it to roll upon a needle, and thus turn the latter round and move a pointer attached to it through a sensible arc.

The figure given below will show the nature of the apparatus.



A small flat rod of the material to be examined, such as brass, iron, or glass, about 30 cm. long, 1 cm. broad, and 2 or 3 mm. thick, is laid upon two wooden blocks, placed about 25 cm. apart. A weight is put upon one end of the rod to keep it from moving; under the other end, at right angles to the length of the rod, is laid a fine sewing-needle, to the eye-end of which a light pointer of straw, about 16 or 20 cm. long, is attached by sealing-wax. Behind the pointer (which is painted black) a screen of white cardboard is fixed on the wooden block by drawing-pins.

When the rod is heated by a lamp-flame, the free end of it, as it expands, moves forward upon the needle and rolls it round, its movement being shown by the motion of the pointer. Even the slight expansion of a slip of glass is thus easily rendered evident to a class.

I have constructed for my own use a double apparatus on the same principle, in which the surfaces between which the needle rolls are of brass, ground true and flat. Two bars of different materials lie side by side, each having its own bit of needle and aluminium pointer, ranging over the same scale. They are heated equally by a broad flame (spirits of wine in a wide trough) and the difference of expansibility as well as the fact of expansion by heat is thus shown.

It is advisable to counterpoise the pointer by putting a shot or two into the lower end of the straw which projects below the needle, and cementing them in by sealing-wax. Also, before the experiment is shown to an audience, it is well to make sure that the needle rolls fairly and freely between the bar and the block. Such precautions, however, are not in the slightest degree necessary for school-work; for there is always one thing which gives the typical boy greater pleasure than to see an experiment succeed, and that is—to see it fail.

Eton College H. G. MADAN

COMPARATIVE STUDIES UPON THE GLACIATION OF NORTH AMERICA, GREAT BRITAIN, AND IRELAND[1]

OBSERVATIONS extending over several years upon glacial phenomena on both sides of the Atlantic had convinced the author of the essential identity of these phenomena; and the object of this paper was to show that the glacial deposits of Great Britain and Ireland, like those of America, may be interpreted most satisfactorily by considering them with reference to a series of great *terminal moraines*, which both define confluent lobes of ice and also often mark the line separating the glaciated from the non-glaciated areas.

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[1] **Abstract of a Paper read at the Birmingham meeting of the British Association, September 1886, by Prof. H. Carvill Lewis, M.A., F.G.S.**

The paper began with a sketch of recent investigations upon the glaciation of North America, with special reference to the significance of the terminal moraines discovered within the last few years. The principal characters of these moraines were given, and a map was exhibited showing the extent of the glaciated areas of North America, the course of the interlobate and terminal moraines, and the direction of striation and glacial movement. It was shown that, apart from the great ice sheet of North-Eastern America, an immense lobe of ice descended from Alaska to Vancouver's Island on the western side of the Rocky Mountains, and that from various separate centres in the Cascade, Sierra Nevada, and Rocky Mountains there radiated smaller local glaciers.

The mountains encircling the depression of Hudson Bay seemed to be the principal source of the glaciers which became confluent to form the great ice-sheet. In its advance, this ice-sheet probably met and amalgamated with a number of already existing local glacial systems, and it was suggested that there was no necessity for assuming either an extraordinary thickness of ice at the Pole, or great and unequal elevations and depressions of land.

Detailed studies made by the author in Ireland in 1885 had shown remarkably similar glacial phenomena.

The large ice-sheet which covered the greater part of Ireland was composed of confluent glaciers, while distinct and local glacial systems occurred in the non-glaciated area. The principal ice-sheet resembled that of America in having for its centre a great inland depression surrounded by a rim of mountains.

These appear to have given rise to the first glaciers, which, after uniting, poured outwards in all directions. Great lobes of this ice-sheet flowed westward out of the Shannon and out of Galway, Clew, Sligo, and Donegal Bays, northward out of Loughs Swilly and Foyle, and south-eastward out of Dundalk and Dublin Bays; while to the south the ice-sheet abutted against the Mullaghareirk, Galty, and Wicklow Mountains, or died out in the plains.

Whether it stopped among the mountains or in the lowlands, its edge was approximately outlined by unusual accumulations of drift and boulders, representing the terminal moraines. As in America, this outer moraine was least distinct in the lowlands, and was often bordered by an outer "fringe" of drift several miles in width.

South of an east and west line extending from Tralee to Dungarvan is a non-glaciated zone free from drift. Several local systems of glaciers occur in the South of Ireland, of which by far the most important is that radiating from the Killarney Mountains, covering an area of 2000 square miles, and entitled to be called a local ice-sheet. Great glaciers from this Killarney ice-sheet flowed out of the fjord-like parallel bays which indent the south-western coast of Ireland. At the same time the Dingle Mountains, the Knockmealdown and Comeragh Mountains, and those of Wexford and Wicklow furnished small separate glaciers, each sharply defined by its own moraine.

No evidence of any great marine submergence was discovered, although the author had explored the greater part of Ireland, and the eskers were held to be phenomena due to the melting of the ice and the circulation of sub-glacial waters. The Irish ice-sheet seemed to have been joined at its north-eastern corner by ice coming from Scotland across the North Channel. All the evidence collected indicates that a mass of Scotch ice, reinforced by that of Ireland and England, filled the Irish Sea, over-riding the Isle of Man and Anglesey, and extending at least as far south as Bray Head, south of Dublin. A map of the glaciation of Ireland was exhibited in which the observations of the Irish geologists and of the author were combined, and in which was shown the central sheet, the five local glacial systems, all the known striæ, and the probable lines of movement as indicated by moraines, striæ, and the transport of erratics.

The glaciation of Wales was then considered. Wales was shown to have supported three distinct and disconnected local systems of glaciers, while at the same time its extreme northern border was touched by the great ice-lobe filling the Irish Sea. The most extensive local glaciers were those radiating from the Snowdon and Arenig region, while another set of glaciers radiated from the Plinlimmon district and the mountains of Cardiganshire, and a third system originated among the Brecknockshire Beacons. The glaciers from each of these centres transported purely local boulders and formed well-defined terminal moraines. The northern ice-lobe, bearing granite boulders from Scotland and shells and flints from the bed of the Irish Sea, invaded the northern coast, but did not mingle with the Welsh glaciers. It smothered Anglesey and part of Carnarvonshire on the one side and part of Flintshire on the other, and heaped up a terminal moraine on the outer flanks of the North Welsh mountains. This great moraine, filled with far-travelled northern erratics, is heaped up in hummocks and irregular ridges, and is in many places as characteristically developed as anywhere in America. It has none of the characters of a sea-beach, although often containing broken shells brought from the Irish Sea. It may be followed from the extreme end of the Lleyn Peninsula (where it is full of Scotch granite erratics), in a north-easterly direction through Carnarvonshire past Moel Tryfan and along the foot of the mountains east of Menai Strait to Bangor, where it goes out to sea, re-appearing further east at Conway and Colwyn. It turns south-eastward in Denbighshire, going past St. Asaph and Halkin Mountain. In Flintshire it turns southward, and is magnificently developed on the eastern side of the mountains, at an elevation of over 1000 feet, between Minera and Llangollen, south-west of which place it enters England. There is evidence that, where the ice-sheet abutted against Wales, it was about 1350 feet in thickness. This is analogous to the thickness of the ice-sheet in Pennsylvania, where the author had previously shown that it was about 1000 feet thick at its extreme edge, and 2000 feet thick at points some 8 miles back from its edge. The transport of erratics coincides with the direction of striæ in Wales as elsewhere, and is at right angles to the terminal moraine.

The complicated phenomena of the glaciation of England, the subject of a voluminous literature and discordant views, had been of high interest to the author, and had led him to redouble his efforts for its solution. He had found that it was possible to accurately map the glaciated areas, to separate the deposits made by land ice from those due to icebergs or to torrential rivers, and to trace out a series of terminal moraines both at the edge of the ice-sheet and at the edge of its confluent lobes. Perhaps the finest exhibition of a terminal moraine in England is in the vicinity of Ellesmere, in Shropshire. A great mass of drift several miles in width, and full of erratics from Scotland and from Wales, is here heaped up into conical hills which inclose "kettle holes" and lakes, and have all the characters of the "kettle-moraine" of Wisconsin. Like the latter, the Ellesmere moraine here divides two great lobes of ice, one coming from Scotland, the other from Wales. This moraine may be traced continuously from Ellesmere eastward through Madeley, Macclesfield, to and along the western flank of the Pennine Chain, marking throughout the southern edge of the ice-sheet of northern England. From Macclesfield the same moraine was traced northward past Stockport and Staleybridge to Burnley, and thence to Skipton in Yorkshire. North-east of Burnley it is banked against the Boulsworth Hills up to a height of 1300 feet in the form of mounds and hummocks. South and east of this long moraine no signs of glaciation were discovered, while north and west of it there is every evidence of a continuous ice-sheet covering land and sea alike. The striæ and the transport of boulders agree in proving a southerly and south-easterly direction of ice-movement in Lancashire and Cheshire.

From Skipton northward the phenomena are more complicated. A tongue of ice surmounted the watershed near Skipton, and protruded down the valley of the Aire as far as Bingley, where its terminal moraine is thrown across the valley like a great dam, reminding one of similar moraine dams in several Pennsylvania valleys. A continuous moraine was traced around this Aire glacier. Another great glacier, much larger than this, descended Wensleydale and reached the plain of York. The most complex glacial movements in England occurred in the mountain region about the Nine Standards, where local glaciers met and were overpowered by the greater ice-sheet coming down from Cumberland. The ice-sheet itself was here divided, one portion going southward, the other in company with local glaciers and laden with the well-known boulders of "Shap granite" being forced eastward across Stainmoor Forest into Durham and Yorkshire, finally reaching the North Sea at the mouth of the Tees. The terminal moraine runs eastward through Kirkby Ravensworth, toward Whitby, keeping north of the Cleveland Hills, and all Eastern England south of Holderness appears to be non-glaciated. On the other hand, all England north of Stainmoor Forest and the River Tees, except the very highest points, was smothered in a sea of solid ice.

There is abundant evidence to prove that the ice-lobe filling the Irish Sea was thicker towards its axis than at its edges, and at the north than at its southern terminus, and that it was reinforced by smaller tributary ice-streams from both England and Ireland. It may be compared with the glacier of the Hudson River Valley in New York, each having a maximum thickness of something more than 3000 feet. The erosive power of the ice-sheet was found to be extremely slight at its edge, but more powerful farther north, where its action was continued for a longer period. Towards its edge its function was to fill up inequalities rather than to level them down. It was held that most glacial lakes are due to an irregular dumping of drift, rather than to any scooping action, observations in England and in Switzerland coinciding with those in America to confirm this conclusion. Numerous facts on both sides of the Atlantic indicate that the upper portion of the ice-sheet may move in a different direction from its lower portion. It was also shown that a glacier in its advance had the power of raising stones from the bottom to the top of the ice, a fact due to retardation by friction of its lower layers. The author had observed the gradual upward passage of sand and stones in the Grindelwald glacier, and applied the same explanation to the broken shells and flints raised from the bed of the Irish Sea to the top of Moel Tryfan, to Macclesfield, and to the Dublin mountains.

The occurrence of stratified deposits connected with undoubted moraines, was shown to be a common phenomenon, and instances of stratified moraines in Switzerland, Italy, America, and Wales, were given. The stratification is due to waters derived from the melting ice, and is not proof of submergence.

It was held that, notwithstanding a general opinion to the contrary, there is no evidence in Great Britain of any marine submergence greater than about 450 feet. It was to be expected that an ice-sheet advancing across a sea-bottom should deposit shell-fragments in its terminal moraine. The broad principle was enunciated that wherever in Great Britain marine shells occur in glacial deposits at high levels, it can be proved both by striæ and the transport of erratics that the ice advanced on to the land from out of the sea. The shells on Three Rock Mountain near Dublin, and in North Wales and Macclesfield, all from the Irish Sea; the shells in Cumberland transported from Solway Firth; those on the coast of Northumberland brought out of the North Sea; those at Airdrie in Scotland, carried eastward from the bottom of the Clyde; and those in Caithness from Moray Firth, were among examples adduced in proof of this principle. The improbability of a great submergence not leaving corresponding deposits in other parts of England was dwelt upon.

It was also held that there was insufficient evidence of more than one advance in the ice-sheet, although halts occurred in its retreat. The idea of successive elevations and submergences with advances and retreats of the ice was disputed, and the author held that much of the supposed inter-glacial drift was due to sub-glacial waters from the melting ice.

The last portion of the paper discussed the distribution of boulders, gravels, and clays south of the glacial area. Much the greater part of England was believed to have been uncovered by land ice. The drift deposits in this area were shown to be the result in part of great fresh-water streams issuing from the melting ice-sheet and in part of marine currents bearing icebergs during a submergence of some 450 feet. The supposed glacial drift about Birmingham and the concentration of boulders at Wolverhampton were regarded as due to the former agent, while the deposits at Cromer and the distribution of Lincolnshire chalk across Southern England were due to the latter. The supposed esker at Hunstanton was held to be simply a sea-beach, and the London drift deposits to be of aqueous origin. Thus the rival theories of floating icebergs and of land glaciers were both true, the one for Middle and Southern England, the other for Scotland, Wales, and the North of England; and the line of demarkation was fixed by great terminal moraines. The paper closed with an acknowledgment of indebtedness to the many geologists in England and Ireland who had uniformly rendered generous assistance during the above investigation.

THE CLIMATE OF NORTHERN EUROPE AND THE GULF STREAM

IN view of the reference made by Sir William Dawson, in his inaugural address at the meeting of the British Association, to the effect of the Gulf Stream on the climate of Northern Europe, particularly that of Norway, and the consequences of a diversion of the stream from its present course, the following contribution to the subject by the well-known Norwegian *savant*, Dr. Karl Hesselberg, which appeared in a recent number of the scientific journal *Naturen*, may be of interest and tend to its further elucidation.

According to the situation of Norway on the globe, the northern part of the country should have a distinct Polar climate, with eternal ice and snow, a home only for the Eskimo and Polar bears. Several circumstances contribute, however, to make it otherwise. The country forms a western promontory of the great Asiatic-European continent, and receives its full share of the advantages of such a situation. Mild south-west winds blow throughout the year, while warm sea-currents wash its extensive shores summer as well as winter. The winter cold is so reduced that only a small portion of the heat of the summer sun is consumed in melting the snow. The length of the summer days, too, which north of the Polar Circle last twenty-four hours, contribute to raise the mean temperature, and accelerate the growth of the flora. Certain other circumstances, as, for instance, the formation of the country and the physico-geographical conditions of the North Atlantic Ocean, contribute equally to make the Norwegian climate one of the most favourable in the world. A brief *résumé* of the circumstances will be of interest.

A chart of the distribution of the atmospheric depression in the North Atlantic Ocean—the Norwegian Sea—shows that all the year round a strong barometric minimum prevails in the middle of the sea between Norway, Iceland, Jan Mayen, and Spitzbergen, the consequence of which is that south-west winds always blow in the eastern part of this area, viz. along the coast of Norway. Warm water is thereby forced up towards Norway and Spitzbergen, even into the East Arctic Ocean. The bottom formation of the sea, too, contributes to preserve the high temperature. If a chart be examined of the depths of the North Atlantic Ocean, such a one, for instance, as is the result of Prof. Mohn's labours after the measurements of the Norwegian North Atlantic Expedition, it will be found that the sea-bottom between Norway, the Faroe Islands, Iceland, and Jan Mayen, forms a basin with a depth of a little over 2000 English fathoms. It will also be seen that the Norwegian coast does not fall abruptly into this abyss, but that the bottom along the whole coast slopes gradually down from the shore seawards to a certain point where it terminates perpendicularly. In other words, Norway is surrounded with a continuous "bank," which in a great measure contributes to preserve the high temperature along the coast. In the great basin, however, the water is icy cold at the bottom, but against this the bank forms a natural barrier, whilst above the bank the warm water is without any bottom layer of cold. It is the warm water which fills the fjords and there preserves a temperature so high that it is sometimes higher than the mean temperature of the air, and under which the fjords do not freeze, a circumstance of great importance. If the temperature of the sea-water in the winter contributes to raise the temperature of the air, it will in the summer have the opposite effect, and cause the climate to be very much tempered along the coast. It is only in the fjords and adjacent valleys that the temperature in the summer rises to a height unusual for the latitude.

In order to show the relatively favourable climate which Norway enjoys, Dr. Hesselberg supplies two diagrams. The first of these shows the mean temperature of the air over Europe and the North Atlantic Ocean in January, when it is lowest. Isotherms are shown for every fifth degree. If now, for instance, the isotherm 0°—the temperature of the air—be followed, it will at once be seen how far it shoots up northwards between Iceland and Norway, in fact, right above lat. 70° N. Instead of running east and west, it goes nearly straight north and south, particularly along the west coast of Norway, which it follows throughout its entire length, from the latitude of Tromsö to that of Christiansand. Hence it deviates towards Denmark, then runs into the Baltic, returns to Hamburg, and thence runs in a south-easterly direction across Europe, nearly down to the Adriatic Sea. Here it first trends eastwards, across Turkey and the Black Sea. Off the Norwegian coast, therefore, in lat. 70° N., the same mean temperature prevails in January as in Southern Europe in lat. 45°, and even there the mean temperature is probably 3° higher than might be expected according to the latitude. The other isotherms have a similar course, as well as the temperature at the surface of the sea. A great wave of warm water rolls up along the coast of Norway, and may be traced even to Spitzbergen.

Another equally interesting illustration of the mildness of the winter in Norway is shown by two diagrams of the "thermal anomaly" in January. By way of comparison the month of July is included. It may be added that by thermal anomaly is meant the difference which exists between the *true* mean temperature of a place and the mean temperature *actually registered* in that latitude.

In January the thermal anomaly is very remarkable. Thus, along the coast of Norway, between the northernmost and westernmost promontories, the North Cape and Stat, it reaches + 20° C., and in the sea outside most probably + 25° C. These figures are certainly very remarkable. Eastwards, it decreases inland, but even here—where the cold is very great in the winter—it never falls below + 7°. In the Baltic, on the other hand, it again rises, as might be expected.

In the summer, however, the conditions are far from being so favourable. There is, indeed, then a narrow strip of land, on the very verge of the coast, where the thermal anomaly is slightly *negative*. The line for the 0° C. anomaly then follows the west coast, decreasing gradually seawards, whilst eastwards, across Southern Norway, it rises to + 4° C., and in Finmarken to + 70° C.

For the further elucidation of this, the following comparison of the January mean temperature in various places on the globe in about the same latitude may serve :—

About 60° *N. lat.*	°
Hellisö Lighthouse...	2
Bergen	0
Christiania ...	− 5
Stockholm ...	− 3
St. Petersburg	− 10
Jakutsk	− 42
North Kamchatka ...	− 20
South Alaska	− 20
Great Slave Lake ...	− 25
North Coast of Labrador ...	− 25
Cape Farewell	− 7
Shetland Islands	4

About 71° *N. lat.*	°
North Cape...	− 4
South Novaya Zemlya	− 20
Mouth of the Yenisei	− 34
Mouth of the Lena	− 40
Point Barrow	− 30
Boothia	− 32
Upernivik ...	− 20
Jan Mayen ...	− 10

The coldest place on the globe where the mean temperature has been *exactly* ascertained, viz. Werchojansk, in the interior of Siberia, with − 48° C. in January, lies in the same latitude as Bodö, where it is − 2° C., and Röst, with 0°·5 C.

In order to obtain correct normal values of the temperature in a place, long and continuous series of observations are necessary; and when we consider that the longest we possess for any place only extends over 100 years, and that meteorology is but a science of yesterday, the Norwegian meteorological records can make a fair show. With regard, however, to the changes which take place in the climate in a certain spot during ages—which occurrence is beyond dispute—we have no reliable data. I will only mention here Prof. Blytt's theory,[1] which has attracted many supporters, viz. that the periodical changes in the climate are due to the precession of the equinoxes (with a mean period of about 21,000 years), and to changes in the eccentricity of the earth's orbit.

It is, however, possible to accept a shorter periodical change in the climate than this, and theories on this point have not been wanting; but the only one which has found any support is the eleven-year period, corresponding to that of the sunspots, which again coincides with that of the terrestrial magnetic phenomena. It has even been attempted to bring the fall of rain and snow within a certain law, and, as some maintain, with success; but in my opinion the proofs advanced in support of such a theory are far from being conclusive.

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[1] Cf. Prof. Darwin's Address to the British Association, Section A; also NATURE, vol. xxxiv. pp. 220 and 239.

TO PROVE THAT ONLY ONE PARALLEL CAN BE DRAWN FROM A GIVEN POINT TO A GIVEN STRAIGHT LINE

(1) LET OP and OQ be two lines at right angles, and let PQ move along them from O, so that OP always = OQ. Then PQ always > OQ or OP.

Hence if OQ increase without limit, PQ must also do so.

Let ON bisect the angle POQ. Then N bisects PQ.

Then if OQ increase without limit, QN does so (QN = ½ QP).

If OQ′ be taken along ON = OQ, QQ′ > QN.

Hence if OQ increase without limit, QQ′ does so.

Similarly by bisecting Q′OQ by OM, we can show that QM increases without limit with OQ, and so on by continual bisection. Hence—

If two straight lines meet at any angle, the perpendicular from a point of one on the other becomes infinite when that point is at infinity.

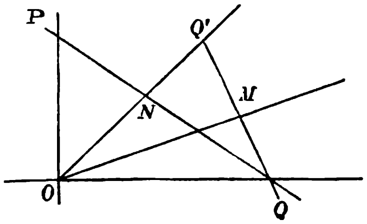

FIG. 1.

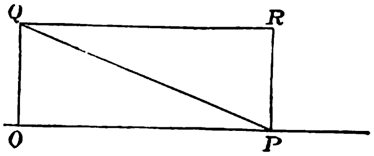

FIG. 2.

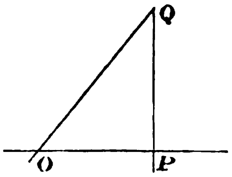

FIG. 3.

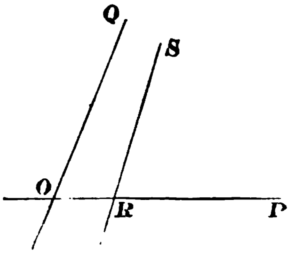

FIG. 4.

(2) Let OQ be some given length taken at right angles to a line OP;

Let PR move along OP at right angles to OP, so that PR always = OQ.

Join QR, QP.

Let OP increase without limit.

Then the angle PQR tends to become zero.

For the lines QR, PQ never become infinitely separated.

Thus there is evidently some definite position for the line QP when OP becomes ∞.

(3) Let a line PQ move at right angles to OP, so that PQ = OP.

Then if OP increase without limit, OQ increases without limit.

Hence, there is some finite angle, QOP, such that the perpendicular QP from Q at ∞ on OP falls at an infinite distance from O.

The same thing is evidently true for all angles less than QOP.

Then either it is true of all angles less than a right angle, in which case it can be easily shown that only one parallel can be drawn from a given point to a given line ;

Or, there is some limiting angle, QOP, for which QP falls at ∞, and for any greater angle (< right angle) QP falls at some finite distance from O.

Let QOP be this limiting angle. Take R on OP, and draw RS to Q at ∞ along OQ.

Then if S is at ∞, the perpendicular SP falls at an infinite distance from R.

∴ Angle PRS not greater than POQ, and it cannot be less (Eucl., I., 16 and 27).

Hence it must be equal.

Hence RS making the angle SRP = QOP meets OQ at ∞ at both ends.

And any other straight line through R becoming infinitely distant from RS must cut OQ in some finite point.

Thus from R only one parallel, RS, can be drawn to a given line, OQ.

By moving OP along OQ always at the same angle, QOP, we can show that

From a given point only one parallel can be drawn to a given line.

This theorem, therefore, must be true. E. BUDDEN

SCIENTIFIC SERIALS

American Journal of Science, October.—A dissected volcanic mountain; some of its revelations, by James D. Dana. Here the author returns to the subject of Tahiti, largest of the Society Islands, already described by him in 1850 from materials supplied by the Wilkes Exploring Expedition of 1839. The old cone, some 7000 feet high, is now a dissected mountain, with valleys cut profoundly into its sides, and laying bare the centre to a depth of from 2000 to nearly 4000 feet below the existing summit. As shown on the accompanying map, the valleys, due to erosion, are so crowded on one another, that the dissection is complete, thus disclosing the inner structure of a great volcanic mountain. The interior is shown to be composed, not of lava-beds, there being no horizontal lines, but of imperfect columnar formations, rising vertically in the unstratified mass quite to the summit. The uniform massiveness through so great a height at the volcano's centre is attributed to the cooling of continuously liquid lava in the region of the great central conduit of the cone. A comparative study of Mauna Loa (Hawaii), shows that such a massive central structure is a common feature of the *greater* volcanic mountains, the extremely slow cooling process under great pressure causing the lava to solidify into a *compact* crystalline rock, and often into a *coarsely crystalline* rock.—Origin of the ferruginous schists and iron ores of the Lake Superior region, by R. D. Irving. Rejecting the igneous theory, now held by few, the writer, after a careful survey of the whole field, concludes that these rocks were once carbonates analogous to those of the coal-measures, which by a process of silicification were transformed into the various kinds of ferruginous formations now occurring in this region.—Further notes on the artificial lead silicate from Bonne Terre, Montana, by H. A. Wheeler. An analysis of this interesting substance, which was found under the hearth of an old reverberatory roasting-furnace, yielded 73·66 PbO, 17·11 SiO_2, NiO 3·06 (coarse crystals), 72·93 PbO, 18·51 SiO_2, and smaller quantities of nickel, cobalt, and other ingredients.—Limonite pseudomorphs after pyrite, by John G. Meem. The paper gives a short account of the pseudomorphs occurring in Rockbridge County, Virginia, where they are associated with Lower Silurian limestones. These crystals, varying in colour from a very light to a very dark brown, and sometimes almost black, are hydrous, and yield a yellow powder, showing them to be limonite, most commonly of octahedral form.—Note on the hydro-electric effect of temper in case of steel, by C. Barus and V. Strouhal. The object of this inquiry is to determine directly the carbon relations of steel as a function of the temperature (0° to 400°, 400° to 1000°) and of the time of annealing, with full reference to the physical occurrences observed in the first and second phases of the phenomenon.—On the crystalline structure of iron meteorites, by Oliver Whipple Huntington. It is shown that the usual classification of these meteorites into octahedral and cubic crystals cannot be natural or fundamental. A careful examination of the large collection belonging to Harvard College, containing types of all the characteristic meteorites of this class, leads to the conclusion that masses of meteoric iron are cleavage crystals, broken off probably by impact with the air, and showing cleavages parallel to the planes of all three fundamental forms of the regular system (octahedron, cube, and dodecahedron); further, that the Widmanstättian figures and Neumann lines themselves are sections of planes parallel to these same forms, exhibited in every gradation from the broadest bands to the finest markings, with no natural break, the features of von Widmanstätten's figures being, moreover, due to the eliminations of impurities during the process of crystallisation.—A new meteoric iron from Texas, by W. Earl Hidden. The specimen here described and illustrated was discovered by Mr. C. C. Cusick on June 10, 1882, near Fort Duncan, Maverick County, Texas. It weighs over 97 pounds, is quite soft, being easily cut with a knife, and consists of iron 94·90; nickel and cobalt, 4·87; phosphorus, 0·25, with traces of sulphur and carbon; specific gravity, 7·522.—On pseudomorphs of garnet from Lake Superior and Salida, Colorado, by S. L. Penfield and F. L. Sperry. The Lake Superior specimen is essentially an iron alumina garnet, with formula $Fe_2Al_2Si_3O_{12}$. That of Colorado is higher in protoxides and water, the increase being perhaps due to the presence of ripidolite.—Further notes on the meteoric iron from Glorieta Mount, New Mexico, by George F. Kunz.—On the Brookite from Magnet Cove, Arkansas, by Edward S. Dana. These crystals, first described in 1846 by Shepard under the name of *arkansite*, are especially remarkable for the great variety of their forms, which is most unusual for crystals occurring in the same locality.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, November 16.—Prof. W. H. Flower, F.R.S., President, in the chair.—An extract was read from a letter addressed to the President by Dr. Emin Bey, dated Wadilai, Eastern Equatorial Africa, January 1, 1886, and containing some notes on the distribution of the Anthropoid Apes in Eastern Africa.—A letter was read, addressed to the Secretary by Dr. Chr. Lütken, of Copenhagen, F.M.Z.S., containing some information as to the locality of *Chiropodomys penicillatus.*—A letter was read from Dr. A. B. Meyer, C.M.Z.S., communicating some remarks by Mr. K. G. Henke on a specimen of a hybrid Grouse in the Dresden Museum.—Prof. Flower, F.R.S., exhibited and made remarks on a specimen of a rare Armadillo (*Tatusia pilosa*) belonging to the Scarborough Museum.—Prof. Bell exhibited, and made remarks on, an object (apparently of the nature of an amulet) made from a portion of the skin of some mammal, and received from Moreton Bay, Australia.—Mr. H. Seebohm, F.Z.S., exhibited a skin of what he considered to be a young individual of the Lesser White-fronted Goose (*Anser albifrons minutus*), shot in September last on Holy Island, off the coast of Northumberland, and observed that it was the first recorded example of the small form of the White-fronted Goose which had been obtained on the coasts of our islands.—Mr. Blanford, F.R.S., exhibited, and made remarks on, a mounted specimen of a scarce Paradoxure (*Paradoxurus jerdoni*) from the Neilgherry Hills in Southern India.—A communication was read from Colonel Charles Swinhoe, F.Z.S., containing an account of the species of Lepidopterous insects which he had obtained at Mhow, in Central India.—A communication was read from Dr. R. W. Shufeldt, C.M.Z.S., containing an account of the anatomy of *Geococcyx californianus.*—Mr. Lydekker described three crania and other remains of *Scelidotherium*, two of the former being from the Argentine Republic, and the third from Tarapaca, in Chili. One of the crania from the first locality he referred to the typical *S. leptocephalum* of Owen, while the second, which had been described by Sir R. Owen under the same name, he regarded as distinct, and proposed to call *S. bravardi*. The Tarapaca form, which was characterised by the extremely short nasals, was also regarded as indicating a new species, for which the name of *S. chilense* was proposed. The author concluded that there were not sufficient grounds for separating Lund's proposed genus *Platyonyx* from *Scelidotherium.*—Mr. G. A. Boulenger pointed out that two distinct forms of the Batrachian genus *Bombinator* occur in Central Europe, and read notes on their distinctive characters and geographical distribution.—A communication was read from Dr. R. W. Shufeldt, containing a correction, with additional notes, upon the anatomy of the *Trochili*, *Caprimulgi*, and *Cypselidæ.*—A communication was read from Dr. R. A. Philippi, C.M.Z.S., containing a preliminary notice of some of the Tortoises and Fishes of the coast of Chili.—Mr. Sclater exhibited the head of, and made remarks upon, an apparently undescribed species of Gazelle from Somali Land.

Geological Society, November 3.—Prof. J. W. Judd, F.R.S., President, in the chair.—Henry Howe Arnold-Bemrose, Richard Assheton, Francis Arthur Bather, Rev. Joseph Campbell, M.A., John Wesley Carr, Thomas J. G. Fleming, Thomas Forster, Edmund Johnstone Garwood, George Samuel Griffiths, Dr. Frederick Henry Hatch, Ph.D., Robert Tuthill Litton, Frederick William Martin, Richard D. Oldham, Forbes Rickard, Albert Charles Seward, Herbert William Vintner, and Charles D. Walcott were proposed as Fellows of the Society.—The following communications were read:—On the skull and dentition of a Triassic Saurian, *Galesaurus planiceps*, Ow., by Sir Richard Owen, K.C.B., F.R.S. The author referred to a fossil skull from the Triassic sandstone of South Africa, which combined dental characters resembling those of a carnivorous mammal with the cranial structure of a Saurian. The structure was described and figured in Owen's "Catalogue of the Fossil Reptilia of South Africa," under the generic title of *Galesaurus*, as belonging to a distinct sub-order of Reptilia termed *Theriodontia*. The characters of the skull and teeth of the original specimen of *Galesaurus* have been brought to light by further development. In both the type-specimen and that lately received, the reptilian nature of the fossil is indicated by the single occipital condyle and other features. The chief difference from a mature male of a placental or marsupial carnivore is the evidence of a primordial "gullet-tract." Further details as to the structure of the skull were given, more especially with reference to the orbits and nasals. The palatal region repeats the same general characters as in previously described Theriodonts. The angle of the jaw is not produced, as in the crocodile, beyond the articular element. In general shape and bony strength the mandible of *Galesaurus* resembles that of a mammal. The dentition is so much better preserved in the new specimen than in the type Galesaur as to call for description and illustration. In four of the upper molars the entire crown is preserved; it shows less length and greater breadth than appears in the previous restoration, is moderately curved externally, and triangular; the base is flanked by a short cusp before and behind, and the corresponding margins are finely crenulate, as in the molars of *Cynodracon*. The incisors are eight in number in both upper and lower jaws, four in each premaxillary, opposed or partially interlocking with the same number in each mandibular ramus; they have longish, slender, simple-pointed crowns. The canines, one on each side of both upper and lower jaws, have the same laniariform shape and size of crown as in the original fossil. In the right maxillary bone the long deeply-planted root is exposed; the corresponding part of the lower canine is similarly exposed in the left mandibular ramus. No trace of successional teeth, as in ordinary Saurians, has been found. Both crocodiles and alligators have two or more teeth of canine proportions; but the author shows how they differ from those of mammalian carnivores and *Galesaurus*. A similar character and disposition of destructive canines is shown by the fossil jaws of the oolitic great extinct carnivorous Saurians, *e.g. Megalosaurus*. In the Triassic Labyrinthodonts the destructive and prehensile laniaries would, by position, rank as incisors rather than canines. In existing lizards the dental series has more uniformity, and the cement-clad roots contract bony union with the jaw-bone. In *Galesaurus* the teeth, besides being distinguished, as in mammals, by their differential characters, are implanted freely in sockets, the cold-blooded character being chiefly manifested in the greater number of teeth following the canines, and in their want of distinction. Lastly, the author remarked on the earlier reptilian character shown by the oolitic mammal *Amphitherium*, and also by the existing Australian *Myrmecobius*. He speculates on the degree of resemblance manifested by the teeth of the old Triassic reptile of South Africa with the exceptional characters of some of the low Australian forms of mammals.—The Cetacea of the Suffolk Crag, by R. Lydekker, B.A., F.G.S. This paper commenced with notices of previous contributions to the subject by Sir R. Owen, Prof. Ray Lankester, Prof. Huxley, and Prof. Flower. In the preparation of a catalogue of the specimens in the British Museum, the author had had occasion to examine the collection of Cetacea from the Crag, not only in that Museum, but also in the Museum of Practical Geology, that of the Royal College of Surgeons, and in the Ipswich Museum, besides visiting the collections at Brussels. In consequence, several additions to the fauna, and also numerous emendations of specific names, were noticed in the paper now laid before the Society. Prof. Ray Lankester's views as to the Diestian affinities of the English-Crag Cetacea were confirmed by this comparison. Detailed notes on the specimens examined and the species identified were given.—On a jaw of *Hyotherium* from the Pliocene of India, by R. Lydekker, B.A., F.G.S. Colonel Watson, the Political Resident in Kattiawar, had recently sent to the author a fragment of a left maxilla with the three true molars, from Perim Island, in the Gulf of Cambay. The specimen belonged to *Hyotherium*, and apparently to an undescribed species, the differences between which and the several forms previously known from various European and Asiatic beds were pointed out. The author also called attention to the peculiar association of types found in the beds of Perim Island, and to the affinities of the genus *Hyotherium* with the recent *Sus* and *Dicotyles* on the one hand, and with the Upper Eocene *Chœropo'amus* on the other.

Physical Society, November 13.—Prof. Balfour Stewart, President, in the chair.—In opening the proceedings, the President referred to the great loss which the Society had recently sustained by the death of Prof. Guthrie, F.R.S., the founder of the Society, and his predecessor in the chair. In the capacity of Demonstrator, Prof. Guthrie contributed materially to the success of the Society's meetings, and his decease is deeply regretted. The President also announced that the Council were considering what steps should be taken to commemorate the late Dr. Guthrie, and that a circular containing their views would be placed before the members in the course of a few days.—The following papers were then read:—On the peculiar sunrise shadows of Adam's Peak, in Ceylon, by the Hon. Ralph Abercromby, F.R.Met.Soc. The author prefaced his description by an extract from a paper on the same subject by the Rev. R. Abbay, read before the Physical Society, May 27, 1876, in which the explanation proposed is that the effects are caused by total internal reflection, as in ordinary mirage, the difference of air-density being, in this case, due to the lower temperature at high altitudes. The author pointed out that Mr. Abbay neglects the difference of density due to elevation, and that his own thermometric observations disprove conclusively any idea of mirage. The chief phenomena observed were: (1) the appearance of a circular rainbow with spectral figures near the top of the shadow of the peak; and (2) a peculiar rising of the bow and shadow, which seem to stand up in front of the observers. Both these effects are traced to the existence of mist-clouds in the vicinity of the shadow. Two dark rays or brushes were seen to shoot outwards and upwards from the circumference of the bow in directions nearly coinciding with the prolongations of the edges of the shadow, when seen projected on the lower mist-clouds, but the author does not attempt to explain this phenomenon. On one occasion a second and outer bow was seen. The times during which the phenomena were visible were too short to permit sextant observations being taken, but the diameter of the inner bow was estimated at 8° to 12°. A totally distinct kind of shadow is sometimes seen from Adam's Peak just before, and at the moment of sunrise, which seems to stand up against the distant sky. The author found a similar effect at Pike's Peak, Colorado, which is visible only at sunset. Mr. G. Griffiths remarked that he had often seen similar appearances in Switzerland. In answer to questions by the President and Prof. S. P. Thompson, the author said the reason why the shadows were seen from Adam's Peak at sunrise, and from Pike's Peak at sunset, was that the configuration of the land on the west side of the former was similar to that on the east side of the latter, both being low, whereas the opposite sides were high, and therefore unsuitable for showing the phenomena. In all cases he believed the appearances were due to the shadows being projected on clouds of suspended matter in the air at various altitudes. He had not noticed whether the colours were reversed in the second bow seen from Adam's Peak, but observed that this bow nearly, but not quite, touched the inner one.—Note on the internal capacity of thermometers, by A. W. Clayden, M.A. (Read by Prof. Reinold, Secretary.) The author proposes to determine the volume, V, of the mercury by measuring the capacity, c, of a detached piece of the same tube of known length, and thence inferring the volume of t degrees of the thermometer tube, the length of which is equal to that of the piece of tube taken. By assuming the value of a (the coefficient of *apparent* expansion of mercury in the particular kind of glass) to be known, the volume of the mercury in the thermometer can be calculated, since $c = t\,a\,V$. Prof. Rücker remarked that there were often considerable differences in the sectional area of different parts of the same tube, and hence the method would probably not be very reliable.—On the motion of the President, a vote of condolence to Mrs. Guthrie in her sad bereavement was passed unanimously.

Royal Meteorological Society, November 17.—Mr. W. Ellis, F.R.A.S., President, in the chair.—The following were elected Fellows:—Mr. B. A. Dobson, Mr. T. Gordon, Mr. H. Mantle, Rev. J. Watson, and Mr. F. Wright.—The papers read were:—The gale of October 15–16, 1886, over the British Islands, by Mr. C. Harding, F.R.Met.Soc. The storm was of very exceptional strength in the west, south-west, and south of the British Islands, but the principal violence of the wind was limited to these parts, although the force of a gale was experienced generally over the whole kingdom. By the aid of ships' observations, the storm has been tracked a long distance out in the Atlantic. It appears to have been formed about 250 miles to the south-east of Newfoundland on the 12th, and was experienced by many ocean steamers on the 13th. When the first indication of approaching bad weather was shown by the barometer and wind at our western outposts, the storm was about 500 miles to the west-south-west of the Irish coast, and was advancing at the rate of nearly 50 miles an hour. The centre of the disturbance struck the coast of Ireland at about 1 a.m. on the 15th, and by 8 a.m. was central over Ireland. The storm traversed the Irish Sea, and turned to the south-east over the western Midlands and the southern counties of England, and its centre remained over the British Isles about 34 hours, having traversed about 500 miles. The storm afterwards crossed the English Channel into France, and subsequently again took a course to the north-eastwards, and finally broke up over Holland. In the centre of the storm the barometer fell to 28·5 inches; but, as far as the action of the barometer was concerned, the principal feature of importance was the length of time that the readings remained low. At Geldeston, not far from Lowestoft, the mercury was below 29 inches for 50 hours, and at Greenwich it was similarly low for 40 hours. The highest recorded hourly velocity of the wind was 78 miles, from north-west, at Scilly on the morning of the 16th; but, on due allowance being made for the squally character of the gale, it is estimated that in the squalls the velocity reached for a minute or so the hourly rate of about 120 miles, which is equivalent to a pressure of about 70 lbs. on the square foot. On the mainland the wind attained a velocity of about 60 miles an hour for a considerable time; but, without question, this velocity would be greatly exceeded in the squalls. In the eastern parts of England the velocity scarcely amounted to 30 miles in the hour. The force of the gale was very prolonged. At Scilly the velocity was above 30 miles an hour for 61 hours, and it was above 60 miles an hour for 19 hours, whilst at Falmouth it was above 30 miles an hour for 52 hours. The erratic course of the storm and its slow rate of travel whilst over the British Islands were attributed to the presence of a barrier of high barometer readings over Northern Europe, and also to the attraction in a westerly direction, owing to the great condensation and heavy rain in the rear of the storm. The rainfall in Ireland, Wales, and the south-west of England was exceptionally heavy. In the neighbourhood of Aberystwith the fall on the 15th was 3·83 inches, and at several stations the amount exceeded 2 inches. Serious floods occurred in many parts of the country. A most terrific sea was also experienced on the western coasts and in the English Channel, and the number of vessels to which casualties occurred on the British coasts during the gale tell their own tale of its violence. The total number of casualties to sailing-vessels and steam-ships was 158, and among these were five sailing- and one steam-ship abandoned, five sailing- and one steam-ship foundered, and forty-two sailing- and two steam-ships stranded. During the gale the life-boats of the Royal National Life-boat Institution were launched fourteen times, and were instrumental in saving thirty-six lives.—The climate of Carlisle, by Mr. T. G. Benn, F.R.Met.Soc. This is a discussion of the observations made at the Carlisle Cemetery. The mean temperature for the twenty-three years (1863–85) was 47°·5; the absolute highest was 95° on July 22, 1873, and the lowest −5°·5 on January 16, 1881. The mean annual rainfall was 29·80 inches; the greatest monthly fall was 7·84 inches in July 1884, and the least 0·30 inches in January 1881. The average number of rainy days was 174.—Results of hourly readings derived from a Redier barograph at Geldeston, Norfolk, during the four years ending February 1886, by Mr. E. T. Dowson, F.R.Met.Soc.—Results of observations taken at Delanasau, Bua, Fiji, during the five years ending December 31, 1885, with a summary of results for ten years previous, by Mr. R. L. Holmes, F.R.Met.Soc.

Anthropological Institute, November 9.—Mr. Francis Galton, F.R.S., President, in the chair.—The election of the following new Members was announced:—G. W. Hambleton, D. F. H. Hervey, W. R. Reid, M.D., R. J. Ryle, M.A., M.B., and W. F. Stanley, F.G.S.—Prof. Flower exhibited some of Dr. Otto Finsch's casts of natives of the Pacific Islands, and made some general remarks on the collection.—A paper by Dr. E. T. Hamy, entitled "An Interpretation of one of the Copan Monuments," was read. In this paper the author traced a resemblance between the symbol found upon a large and regular convex stone at Copan and the Chinese "Taï-Ki," and argued that the presence of such a symbol in the ruins of Copan, where there exist so many manifestations of a strange and curious art so closely allied to the Eastern arts of the Old World, furnishes a fresh proof in support of the theory of an Asiatic influence over American civilisation.—An exhaustive paper by Mr. H. Ling Roth, on the aborigines of Hispaniola, was read.

SYDNEY

Linnean Society of New South Wales, September 29.—Prof. W. J. Stephens, M.A., F.G.S., President, in the chair.—The following papers were read:—A revision of the Staphylinidæ of Australia, part ii., by A. Sidney Olliff, F.E.S., Assistant Zoologist, Australian Museum. This part, containing the members of the sub-family, *Tachyporinæ*, is another contribution to a general revision of the family. No marked Australian forms have been found, and the new forms are of the ordinary type. The genera *Tachyporus*, *Tachinus*, and *Bolitobius*, are added to the Australian fauna. With this instalment is issued the plate (vii.), which would have accompanied the first part, but for an unfortunate accident to the artist.—Notes on the bacteriological examination of water from the Sydney supply, No. I., by Dr. Oscar Katz.—On a remarkable Bacterium (Streptococcus) from wheat-ensilage, by Dr. Oscar Katz. This paper contains a brief description of a Micrococcus (Streptococcus), obtained from a sample of mouldy wheat-ensilage which, some time ago, it will be remembered, came under public notice in connection with an epidemic which attacked some horses at Coonong, N.S.W. This micro-organism shows characteristic features in its pure cultivations on or in different nutrient soils. Inoculations of this and other microbes found in the samples are intended to be made shortly upon living animals.—Notes on *Lindsea trichomanoides* and *Eriostemon Crowei*, by the Rev. W. W. Woolls, M.A., Ph.D. Dr. Woolls makes some remarks on the first of these, a fern common in New Zealand, but not recognised until of late in New South Wales. He also exhibited a specimen of *Crowea exalata* (*E. Crowei*, v. M.) from the Currajong, and showing marked differences from the *C. saligna* of the flora. Mr. Bettche, however, of the Botanic Gardens, had collected a specimen which was distinctly intermediate, and which probably may unite the two species, *E. salignus* and *E. exalatus* again, according to the Baron's first determination.—Note on a Labyrinthodont fossil from Cockatoo Island, by Prof. Stephens, M.A. The President read a notice of a fossil Labyrinthodont, probably *Mastodonsaurus* sp., recently found at Cockatoo Island, and pointed out the conclusions to which this fossil, the *Ceratodus* of Queensland, and the *Hatteria* of New Zealand, lead in regard to the ancient geographical conditions of the southern hemisphere.—Notes on Australian earthworms, part ii., by J. J. Fletcher, M.A. In this paper descriptions are given of nine new species of earthworms, of which five are indigenous to New South Wales, one is supposed to have been introduced from the Mauritius, two are from Queensland, and one is from Darnley Island, Torres Straits. They include a new species of Perrier's genus *Digaster*, a new species of *Cryptodrilus*, and seven species of *Perichæta*. The last-named are separable into two well-marked groups: the one characterised by the possession of complete circles of setæ, and by the presence of two cæcal appendages of the large intestine in segment xxvi.; the other characterised by having incomplete circles of setæ, and no intestinal cæca. To the first group belong the species from North Queensland and Darnley Island; and the introduced species. Remarks are also made upon a few worms from Percy Island, which were collected during the Chevert Expedition, and which are now in the Macleay Museum, but are immature or not sufficiently numerous to admit of satisfactory description.—Notes on some New South Wales fishes, by Dr. Ramsay, F.R.S.E., and J. Douglas-Ogilby. The common Jew Fish of Port Jackson is here described under the name of *Sciæna neglecta*, the authors pointing out the marked differences between it and *S. antarctica*, Castelnau, and *S. aquila*, Lacep., the species to which it has been hitherto referred. Evidence is also given that *Callionymus reevesii*, Rich., is not, as has been stated, the female of *C. curvicornis*, C. and V.

PARIS

Academy of Sciences, November 15.—M. Jurien de la Gravière, President, in the chair.—Letters having been read from M. de Freycinet announcing the death of M. Paul Bert, Resident-General in Annam and Tonquin, and Member of the Academy, the President and M. Vulpian followed with some remarks on the great services rendered to science by this distinguished physiologist. Reference was made more especially to his researches on the action of light on living organisms; on the physiology of respiration; and on the influence exercised on man, animals, plants, and ferments, by increased or diminished pressure of atmospheric air, of carbonic acid, and of oxygen.—Observations of the small planets made with the large meridian instrument of the Paris Observatory during the second quarter of the year 1886, communicated by M. Mouchez. Numerous observations made by M. P. Puiseux on Pallas, Juno, Olympia, Electra, Urania, Europa, and several other minor planets, are here brought into relation with the ephemerides either of the *Nautical Almanac*, the *Bulletin Astronomique*, or the Berlin *Jahrbuch*.—Researches on the phosphates, by M. Berthelot. Fresh researches are here reported on the double decompositions which reveal in the insoluble tribasic phosphates the existence of two distinct states: one colloidal, amorphous, unstable, answering to the manifold constitution of the soluble phosphates; the other crystallised and stable, in which the three basic equivalents seem on the contrary to play the same part. The phosphates of soda, magnesia, baryta, lime, manganese, and the tribasic phosphates of strontian are specially considered.—Observations of Winnecke's comet, by M. L. Cruls. As observed during last September at the Observatory of Rio de Janeiro, this comet presented the appearance of a nebulosity about 2′ in diameter, without clearly-defined nucleus, of somewhat circular form and slight luminous intensity.—Note on Abel's theorem, by M. G. Humbert.—On the flow of a gas penetrating into a receptacle of limited capacity, by M. Hugoniot. The question here dealt with is to determine the time required to fill a receptacle containing air at an initial pressure p_0, and placed in communication with a reservoir maintained by compressing-engines at a constant pressure $p_1 > p_0$. The reading of the paper was followed by some remarks by M. Haton de la Goupillière on this fresh confirmation of his own theories on the flow of gases.—On the variation of the magnetic field produced by an electromagnet, by M. Leduc. Reference is made to M. Marcel Deprez's communication of October 26, which partly confirmed the conclusions already arrived at by the author, and announced to the Société de Physique on February 19, 1886. But the results obtained present considerable numerical differences, which may be due to the different conditions under which the experiments were made.—On the specific inductive power and conductivity of dielectrics: relation between conductivity and absorbing power, by M. J. Curie.—On the velocity of dissociation, by M. H. Lescœur. It is shown that the results drawn from the velocity of dissociation may supply valuable data regarding the presence of the hydrates and analogous compounds; but they can give no absolute or relative indications respecting the tensions of dissociation.—On some laws of chemical combination, by MM. de Landero and Raoul Prieto. In these studies, of which a few preliminary essays are here communicated, chemical combination is regarded as resulting from the shock of a collision between the particles of the elements forming any given compound. The velocity of the particles in motion being considered as a characteristic constant of each body, the loss of energy or of vital force due to the shock between non-elastic particles is regarded as the equivalent of the quantity of heat liberated by the fusion.—On some histological peculiarities of the acephalous mollusks, by M. Louis Roule.—On the typical nervous system of the ctenobranch mollusks, by M. E. L. Bouvier.—On platyrhinism in a group of African apes, by M. A. T. de Rochebrune. It is shown that the family of the Colobi forms a marked exception to the general rule that the apes of the Old World are all catarhinous. As already anticipated by Dahlbom and Gray, they prove to be distinctly platyrhinous, like all the American Simiæ.—Experimental researches on the synthesis of the lichens in a medium destitute of germs, by M. Gaston Bonnier. The researches carried out by the author since 1882 have resulted in the complete reproduction by synthesis of a certain number of species of lichens under conditions fully confirming the views generally held regarding the complex nature of these vegetable organisms. The results clearly show that a lichen is formed by the association of an Alga and a fungus.—The avifauna of the Mentone caves, by M. Émile Rivière. Of the forty-two species found in these caves, all still survive except *Pyrrhocorax primigenius*, but their present distribution mostly differs from that of Quaternary times, many having disappeared from the Mentone district, owing to climatic changes, the destruction of forests, and the chase.—On the Jurassic Echinidæ of Lorraine, by M. G. Cotteau. The researches made by the author in this branch of palæontology show that in Lorraine the Echinidæ followed the same line of development as in other Jurassic regions.—A physiological study of the respiratory function in singers, by M. Anatole Piltan. Observations made in various institutes show that the quality of the voice is inherent to the expiratory type adopted by the subject, whether unconsciously or acquired by special training.—Bacteriological studies on the Arthropods, by M. Balbiani.

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INDUSTRIAL EDUCATION IN AMERICA

Industrial and High Art Education in the United States. Part I. "Drawing in Public Schools." By J. Edwards Clarke, A.M. (Washington: Government Printing Office, 1885.)

CHANGES required in school training, in the substance as well as in the method of it, are now felt to be a vital question to the political economist and the law-maker, as well as to the moralist. While the old apprenticeship system was in its vigour, the youth was taught at school the three R's and whatever other branches of a liberal education his parents could afford, and for seven years after that technical instruction was given to him in all the branches of the trade he had chosen by his master, the best teacher that could be found in those days. But under the influence of machinery that system has completely collapsed, and the feeling is rising everywhere that something must be done at school to replace instruction given of old by the master. Theorists insist that nothing short of a technical school, where each trade is taught from beginning to end, will sufficiently replace the care and the interest of the latter, and they hold up the Russian Strogonoff school as an example of their being taught in this complete way and triumphantly compare its work with that of the best manufacturing countries. They urge that in a system of public education like that of the United States it is a serious fault that, while a classical or professional education is provided free for the youth who desires it, technical instruction is denied to a far larger body of mechanics who have as perfect a claim to the education they require. In Stockholm the experiment of every elementary school having a carpenter's and joiner's shop attached is being tried, but the impracticability of carrying on in every town schools where instruction in each art can be efficiently given to the labouring classes has left the teaching of theorists little else but theory, and a technical school giving instruction in the one or two principal trades of a district is all that can be looked for.

One item of education, however, has made its way in most European countries, as being a help to all technical work, encouraging observation and correctness, and enabling such observation to be registered and expressed. It is here asserted to be a qualification for nine-tenths of the occupations into which all labour is divided, and is welcomed by the most advanced supporters of technical schools as the first step. Reading, writing, arithmetic, and drawing are now to be the four fundamental studies. A knowledge of it is essential in many of the studies in the schools of science, and especially useful to all engaged in the profession of teaching. More doubtful assertions are that like other technical teaching it does not necessarily interfere with or hinder other work, and that it positively assists in learning to write; this latter having the authority of the London School Board, as well as that of an American writer quoted in this volume. The rise in value also in the labour-market of each mechanic who has the power to draw or even understand a drawing of a mechanical arrangement is often insisted upon; but when such an accomplishment has become common to all, and therefore gives its possessor no superiority, this is rather doubtful, though the raising of a whole class to the capacities of artists and engineers will very likely add to republican equality.

While England has for years encouraged the teaching of drawing, and, the year before last, made it a part of the education of all boys in elementary schools, in the United States a sense of its importance has been only slowly making its way. Recently, however, the Senate requested all information on the subject of industrial and high art education in the United States to be laid before it by the Education Bureau. This work was committed to Mr. J. Edwards Clarke, already the author of a Circular on the subject published in 1874, which excited so much interest and drew so much further information that it is reproduced at p. 487 of this volume, having now, the author claims, some little historical interest, the meagre list which it contains of art institutions in the country at that date contrasting with the changes already brought about. The Bureau had already decided to prepare a much more comprehensive work, which should combine a history of the earliest efforts of writers of all views in all parts of the United States and in England; an account of their failures and successes, and especially of the Massachusetts success; with information as to planning schools of high art and public art-museums; lists of art-publications and materials; extracts from foreign official reports as well as from other foreign material. This, even before the Senate's Commission enlarged its scope, was sufficient to make a tolerably voluminous work. But a source of disorder and much repetition has been a series of delays in its publication. It was complete for publication in 1877, and while in one article of that date criticising the few artistic buildings which New York could show in 1875, it rejoices in a later one at the improvement there in 1883, and the artistic taste displayed by its architects; it was ready again in 1880; corrected again for 1882, and statistical tables down to 1881-82 printed, which again it is promised shall be supplemented by tables reaching down to June 30, 1885, at the end of the fourth volume to which this work is to reach. It was printed in 1885, and inserts publications of that year, yet quotes from an "unpublished" report of the National Educational Association held in 1884; and it is still only promised to the public at this year's end. Since this one volume extends to 1100 pages of, for the most part, small-printed matter, the whole work may be looked upon as an encyclopædia of information bearing upon the drawing question, of value chiefly to two classes of men, viz. school teachers, who will find nothing wanting, and earnest advancers of art education. For unless the general public in America be far different from our own they will not be *led* into the study of the subject by such a publication as this, and in its present form it must quite fail of the general effect upon them aspired to on p. xxx. The fourteen papers by the compiler with which the work opens would be appropriate for rousing attention if dispersed over the country in the most handy form; in their present position their inflated and impetuous style is inconsistent with the idea of exactness to be expected in this class of publication.

As late as 1876 the introduction of drawing into the public schools was looked upon as a novel project in the United States; for while Mr. Clarke considers that England has spent "an enormous aggregate of money in the work" the Americans, so profuse in other educational expenditure, have been strangely apathetic in the matter. One explanation given is that during the Middle Ages the Church and the aristocracy were the great patrons of high art, and this bred an instinctive dislike to its pursuit in the minds of New England emigrants. But besides the delineation of Nature and of all her forms of beauty, the minister of cultivated wealth and luxury, there is another branch of drawing of the highest importance to nearly every mechanic in these days, viz. geometrical drawing, the foundation of all industrial art, leading up to the elaborate perspective of a complicated machine. Both branches are of course required in many manufactures. Which shall be pursued with most energy in any town must depend upon its staple trades; in some few businesses, as in watches and woven fabrics, the mechanical and the ornamental have about equal claims. The same idea, that drawing meant ornamental art alone, and that its chief results would be the sort of things that accomplished young ladies bring home after a few terms of learning drawing, established itself in the minds of the ratepayers. To meet this the general tone of quotations through this report is that "industrial drawing is of the most practical nature, and has nothing to do with pictures of old ruins, landscapes, &c." Yet elsewhere Mr. Clarke is most contemptuous towards any who wish to confine the drawing taught in any school to "that part of it directly related to industrial interests"; and the teaching of the self-willed Haydon, of whom he gives a long account as the victim of cruel and ignorant persecution of "aristocratic connoisseurs" but as an apostle of art to the common people, is just as confidently quoted with no qualification. Haydon's teaching is that the study of the nude human figure is the best qualification of an artist for any manufacturing business, and every one holding a different opinion is dismissed by Mr. Clarke with contempt. This necessity for high art teaching is a hard doctrine, and certainly discouraging to those who hope to qualify a majority of the working classes for artistic producers or intelligent machinists. Perhaps it only resolves itself into the explanation given on p. 482 by Mr. Sparkes, and supported also by the quotation from Mr. William Morris, that the greater includes the less, and that if an artist is well able to delineate the "subtle lines" of the human figure in a complex attitude, he is not likely to fail in working up a lily or a rose; and this is only in accordance with Mr. Stetson's teaching quoted on p. 649. Still a superiority of French over German art designs is attributed to the former making the human figure their first study and then proceeding to flowers and ornament, while the latter take what seems to us the more natural course of the reverse order.

In 1870 the State of Massachusetts after inviting various experts to express their opinions (here reported) decided that drawing should be taught in all its schools. The larger part of this bulky volume is directly or indirectly the history of the call of Mr. Walter Smith, head master of the School of Art at Leeds, who was recommended by Sir Henry Cole to be intrusted with the management of the whole matter. There are 120 closely-printed pages devoted to his work in Massachusetts as Art Director from 1871. Every part of the subject was under his guidance, and to impress upon the reader the amount of work entailed upon him an additional chapter is added to describe the unsatisfactory state of things in Boston before his arrival. The whole of his first report for 1872 is given, and long extracts from each yearly report on normal schools and every other department afterwards. Plans of instruction for evening classes which he superintended, as well as of teaching in school hours, are quoted in full. To advance his subject he took to the principal towns in the State a travelling museum of models and examples for study, many supplied from South Kensington. In one appendix are given copious extracts from an address delivered by him to the Pennsylvania Legislature in 1877 on behalf of the Museum and School of Industrial Art of that State; in another practical papers on drawing, chiefly by him, of value of course to managers who have just succeeded in introducing the teaching of drawing; three lectures delivered respectively to the teachers of the three grades of elementary, grammar, and high schools; followed by extracts from similar addresses delivered, after his connection with Massachusetts had ceased, at Montreal and Quebec.

The chief difficulty of course in setting such a work going was the scarcity of teachers, and this remained a difficulty up to the last. A paper accordingly by General Francis A. Walker, describing drawing as the foundation of all technical education, urges that the normal school should in truth precede, not follow, the elementary school. Another difficulty which an Association of Teachers found, and which showed the state of things at that time, was that there was so great a scarcity of art-books in the country that the Association set itself to encourage the reprinting, translation, and publication of such books.

On the whole, the energy inspired and the method introduced were so successful in Massachusetts that we are told that its history will form a lasting monument alike to the genius of Walter Smith and to the far-reaching foresight of the school authorities and State Legislature in 1872–73.

Appendix D is an account of the differences which rose between the Committee of Education in Massachusetts and Prof. W. Smith. The latter has now returned to England and taken the head mastership of the Art Department of the Technical College at Bradford. Great regrets are expressed at the, to them, untimely event of his resignation, and it is lamented that he should return from leading the industrial education of a continent to an English provincial college! The work now (1883) is reported as in too few hands, though still progressing from the impetus it had received.

No other States have gone into art education with the energy which Massachusetts displayed in 1872. Two others only, New York and Maine, have required that it shall be taught in all schools. In the latter it is urged as the more important, because every natural feature of the country points it out as the seat of manufactures and not of agriculture. But neither Maine nor New York has provided a normal school for the training of teachers. In various other States, however, individual cities have adopted drawing and made it a regular part of the course.

Syracuse, in the State of New York, can boast a priority in this good work even over Boston.

Though speaking everywhere most bitterly of England, —an Americanism so out of date now that happily it is more comical than irritating, especially when he goes so far as to call the great works on political economy which have made their way over the whole civilised world "emissaries of English policy which she has succeeded in introducing,"—the writer everywhere holds up England as an example in art education. The whole of the work is credited to Sir Henry Cole and South Kensington, although there were twenty Government-supported Schools of Design in England in 1847. Still the writer cannot resist the sneer that there would have been no art-teaching in England if a Royal Prince had not urged it! But many times over he relates how visitors were struck with the clumsy inartistic style of all English art-work at the Exhibition of 1851 compared with that of many foreign nations, and the good result of an energetic and most successful effort by the nation to remedy it is constantly urged as appearing at the Philadelphia Exposition of 1876. There the Americans found themselves as far behind England as England had been behind other European countries in 1851, through the inartistic ignorance of their manufacturing classes. Many practical lessons and suggestions were there supplied to them, and much of this volume is a record of their influence. Appendix E gives a lengthy paper by Mr. Stetson reviewing the work exhibited there by all the various foreign nations and by each of the American towns, and it records the influence of this Exhibition upon industrial art. We should like to have heard something, however, of the result of the New Orleans Exhibition, no report of which has reached us, although so much was promised.

While anxious by making drawing general to "utilise all the pleasure which a slate and pencil give a child," Mr. Clarke's unqualified love of liberty makes him object to infringing on even a child's freedom, and actually trusts to the extra interest that many gutter children would take in gaining technical skill to render compulsion unnecessary. He urges with good reason that nowhere would artistic skill be so well rewarded as in the United States during its present rapid rise in wealth as well as in population, and that skilled art labour is far more valuable than the labour bestowed upon plainer, rougher work. He does not however, in his promises held forth to all alike who learn to draw, appear to realise the division of labour between the designer and the numerous mechanics who carry out the artist's ideas on the machine, but seems to look upon all artistic work as carried out single-handed from the design to the article ready for sale. No doubt it is here, as General Walker (already quoted) remarks, that there are boys that have genius in their eyes and fingers instead of a memory and quickness at book-training, who would profit by artistic training. Many such specially gifted artists have already made their mark in America both in architecture and in engraving; the standard of magazine illustrations having been raised even in England by competition with American productions. A larger class whose labour art-education makes valuable are women who are anxious to secure to themselves an independence. They are the principal teachers of drawing in all its branches, and find an excellent outlet for talent. Many artistic trades are also now carried on successfully by them; an account is especially given in Appendix E of the wood-carving taught at a women's school in Cincinnati introduced there by an English workman of the name of Fry. Ladies there, among others, make it a pursuit with great success.

Besides other papers incidentally referred to in our above remarks, various writings of considerable length and of dates from 1845 to 1884 are given in Appendixes A, C, and E, all urging the importance of art education, and instructing those engaged in teaching it.

Appendix F consists of 70 closely-printed pages giving an account of South Kensington, its officials, history, Art Training School, Museum, Art Library; art examples, books, and casts; with the reports for 1882 and 1884, and copious extracts from the Art Directory to show in detail the conditions and regulations under which "aid" is granted in England. Some of the quotations in this appendix are taken from the Directory of 1885. Mr. Clarke assures his countrymen that "in its appointments, and influence on art industrial education, South Kensington Museum stands without a rival. It is a wonderful centre of educational energy." "Other countries, even France, are giving it their official indorsement by modifying their art industrial instruction as rapidly as may be, and bringing it more into harmony with that of the English."

The final Appendix, H, claims to be a fitting end to this volume, and a foreshadowing of the contents of the future volumes. It is Lord Reay's address to the International Educational Conference at the Health Exhibition in 1884.

The printing of this volume is far from so correct as might be expected in a Government publication on Education.

W. ODELL

OUR BOOK SHELF

American Journal of Mathematics. Vol. IX. No. 1. (Baltimore, October 1886.)

WE are glad to note that the successive parts now appear with praiseworthy regularity, and the arrival of our number can be predicted to a very close order of approximation. The volume opens with a continuation of Prof. Sylvester's lectures at Oxford on "The Theory of Reciprocants." The story is resumed with the eleventh and proceeds to the close of the sixteenth lecture. For the cumbrous terminology "projective reciprocants" or "differential invariants" the lecturer now suggests "principiants." From Lecture xiv. the abstract is devoted to the theory of pure and projective reciprocants, or rather principiants, and here we are introduced to the existence and properties of the protomorphs of invariants and reciprocants with which Mr. L. J. Rogers, one of the lecturer's audience, has made us elsewhere familiar. For an account of Dr. Story's new method in analytic geometry, we refer our readers to the author's own description. Dr. F. N Cole gives a full review in Klein's Ikosaeder of what that eminent mathematician has done in his "Vorlesungen über das Ikosaeder und die Auflösung der Gleichungen vom fünften Grade" (1884), and in his "VergleichendeBetrachtungen über neuere geometrische Forschungen" (1872). In Prof. Greenhill's paper on wave-motion in hydrodynamics the writer states that "one of the most important applications of the theory of hydrodynamics is to the question of the motion of waves under gravity and other causes," and his object is "to collect together the chief results hitherto obtained, and to give also a general connected account of the mathematical theory, at the same time attempting to develop it in some directions."

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Longitudes in Brazil

Le numéro du 18 novembre de Nature publie un article du professeur Young sur les progrès de l'astronomie depuis dix ans, dans lequel il est dit que les observations de longitude télégraphiques des officiers américains ont corrigé une erreur de 8·54s. sur Lisbonne, et une bien plus étonnante encore de 35s. sur Rio.

Il y a là une grosse erreur inexplicable de la part du professeur Young, contre laquelle je dois protester comme auteur des cartes hydrographiques du Brésil encore employés aujourd'hui, et auteur de toutes les déterminations géographiques relatives et absolues faites douze ou quinze ans avant la mission américaine de MM. Green et Davis pour les longitudes télégraphiques entre le Brésil et l'Europe.

Sur les mille lieues de côte du Brésil la mission américaine a déterminé six longitudes entre le Para et Buenos Ayres. Voici la comparaison des résultats obtenus par MM. Davis et Green, à l'aide du télégraphe, et par moi, à l'aide de chronomètres et d'observations astronomiques directes. Les observations américaines sont publiées dans le numéro 59 (1880, je crois) "Hydrographic Notice," et les miennes dans les "Annales hydrographiques, 1866."

	Para			Pernambouco			Bahia		
	h.	m.	s.	h.	m.	s.	h.	m.	s.
Long. télégraphique.	3	23	20·94	2	28	48·6	2	43	29·6
Long. Mouchez ...	3	23	18·67	2	28	47·5	2	43	26·9
Erreur			– 2·27s.			– 1·1s.			– 2·7s.

	Rio			Montevideo			Buenos Ayres		
	h.	m.	s.	h.	m.	s.	h.	m.	s.
Long. télégraphique.	3	2	2·3	3	54	9·9	4	2	49·9
Long. Mouchez ...	3	2	0·1	3	54	9·4	4	2	49·9
Erreur			– 2·2s.			– 0·5s.			0·0s.

Il résulte de ce tableau que la plus grande erreur que j'ai commise est – 2·7s. sur Bahia. A Rio l'erreur est de – 2·2s., et non de 35s. comme le prétend M. Young. Dans le Rio de la Plata l'erreur a été trouvée nulle.

Je ne crois pas qu'aucune étendue de côte de mille lieues eut jamais présenté moins d'erreur absolue ou relative que la côte du Brésil après la publication de mes cartes et de mes observations.

Quant à l'erreur sur Lisbonne je l'avais signalée depuis plus de trente ans, elle était connue.

Je vous serais très obligé de vouloir bien publier au moins le tableau comparatif des longitudes que j'ai l'honneur de vous envoyer aujourd'hui, pour protester contre l'erreur qui m'est indirectement imputée.

Veuillez agréer l'assurance de ma parfaite considération.

E. Mouchez

Cooke's "Chemical Physics"

I am told that I have been the object of severe strictures in your journal for republishing my old "Chemical Physics" as if it were a new book. It is a sufficient answer to say that the book was stereotyped when first issued in 1860, and that there has never been any pretence on my part that it has been revised since. I find, on inquiry, that the American publishers have made, since the first edition, three reprints from the plates, and have called these reprints second, third, and fourth editions, changing, with each issue, the date on the title-page; a usage which I regard myself as reprehensible, but which must be sanctioned by the trade since it is so universally followed. All this time, however, the date accompanying my signature after the preface, and the date of the copyright, have remained unaltered. I had supposed the book entirely out of print; and the last reprint of a very few copies to meet a small demand still existing, chiefly in England, was made entirely without my knowledge or consent. On its very face the whole aspect of the book is antiquated; but in it there was brought together certain positive knowledge in connection with the weighing and measuring of aëriform matter, derived chiefly from the classical researches of Regnault, which is still of great importance and not readily found elsewhere; and this is, unquestionably, the reason of the continued demand for a compilation made more than twenty-five years ago. I have, until within a few years, had the expectation of revising the book and presenting the old facts in their new dress, but the failure of my sight has obliged me to give up the plan, and younger men must do the work.

Josiah Parsons Cooke

Cambridge, U.S.A., November 16

Note on Mr. Budden's Proof that only One Parallel can be drawn from a Given Point to a Given Straight Line

Mr. Budden's paper in the last number of Nature (p. 92) is full of inaccuracies of a more or less serious character. Without pointing out these, I wish to show that the essential idea which underlies his reasoning is altogether wrong, as it is based on the "infinite," which he introduces in the most innocent manner by letting his figure grow without limit, and about which he then calmly reasons as if he still dealt with a finite figure. If we let a quantity "increase without limit," we get a quantity which has increased beyond our comprehension, and no one in his senses will wittingly and seriously draw conclusions from what he does not comprehend. Here we might stop, were it not that the constant use in modern mathematics of the infinite (both the small and the great) has made us so familiar with it that an attempt to base an elementary proof on it might seem to many a very natural thing.

In algebra, the infinite number is shown to have one property which we can comprehend, viz. that its reciprocal is zero; and with this property alone we work safely.

In modern geometry, on the other hand, the infinite is used as a kind of shorthand, which enables us to make long statements short, and, at the same time, general. Taking the axiom about parallels for granted, it is shown that all points at an infinite distance in a line may be taken to be one point as far as constructions at a finite distance are concerned. For all lines joining a fixed point, P, to any point at infinity in a line may be taken as parallel to this line, and therefore as coincident. To express this more shortly, it is said that the whole indefinite and infinite part of a line which is out of the reach of our comprehension plays for us only the part of a single point, and accordingly it is called a "point," viz. *the point at infinity* of the line. Similarly it is shown that all points in a plane which are at an infinite distance may be considered as lying in one line, which is then spoken of as *the line at infinity* in the plane, and which is freely and safely used in deducing theorems and solving problems.

If, then, a line in a plane be moved to infinity, making always a given angle with a fixed line, it will ultimately become coincident with—which here means indistinguishable from the line at infinity. The latter then makes with the fixed line a given angle. But this angle may be anything. Hence the "line at infinity" makes any angle we like with any given finite line; in other words, it makes no definite angle at all with it.

It follows, if we take a property of a figure which depends upon the magnitude of an angle, that this property will not necessarily any longer hold if one of the limits of the angle be moved to an infinite distance; for then this angle has not any longer a definite magnitude. To base any reasoning on that property after the figure has been indefinitely increased must therefore necessarily be fallacious. But this is exactly what Mr. Budden does. His proof is based on the implied assumption that if a figure in a plane be increased indefinitely, we can still reason upon it as if it were finite. He may take this as an *axiom*, but then he has *replaced* Euclid's axiom by another, and has not *proved* it; and the question would arise, Which form of the axiom is preferable? I prefer Euclid's.

O. Henrici

Lunar Glaciation

I trust you will allow me a small space to explain regarding this theory of lunar glaciation, referred to by Mr. Darwin in Nature (vol. xxxiv. p. 264).

First, I must thank him for the remarks made, and say that I certainly was not aware that Capt. Ericsson had been at work in the same direction some ten years or more before me.

I laid the theory before the late Prebendary Webb a few years ago, and some selections from it were published in the *Journal* of the Liverpool Astronomical Society, and, being necessarily incomplete, the extracts were not very intelligible. I have never attempted the settlement of the lunar surface temperature, which is quite beyond me, leaving the same in the hands of Prof. Langley, and have confined myself to the solution of the peculiar and unearthly surfacing we see. This I find best explained by glaciation, under conditions of intense cold, say – 60° or 80° C., and absence of all gaseous atmosphere.

I quite indorse Capt. Ericsson's conclusions as to the extreme unlikelihood of such a small globe being finally surfaced by igneous agencies, after it had seas of water, atmosphere, and probably *polar caps*.

Neison, in his "Moon," page 41, line 7, distinctly implies that this took place, *i.e.* "that this high temperature could only arise after the practical disappearance of bodies of water from the lunar surface," the rise in lunar temperature being due to solar heat.

I cannot follow Neison in this, and, on the contrary, believe that the temperature has steadily, if slowly, declined, from a period when there was erosion, with air and water. Polar caps then formed, as on our earth and Mars, and extended as the temperature fell, until at last the entire globe was cased in ice, the last portions to glaciate being what we call the equatorial seas.

Like Capt. Ericsson, I look on the craters and walled plains as having been lagoons of water, left here and there as glaciation extended, at places of greater depth, or more likely as submarine volcanic vents, for we see their sites as craterlets and cones after final glaciation.

The aqueous vapour given off from these lagoons would form a local dome-shaped atmosphere that would retard explosive ebullition, and on its reaching the outer limit of critical temperature, would condense and fall as snow; what fell beyond the lagoon margin would pile to form the ring, and the lagoon surface or flow be gradually lowered by its removal.

But I cannot follow Capt. Ericsson in supposing that the water had a centrifugal motion, and acted as a gigantic carving-tool, that sculptured the enormous terraces in Tycho, Theophilus, &c. On the contrary, I look on it as a quiet process, and that all the circular forms, from small craterlets to even such forms as Mare Crisium or Imbrium, with its huge maritime ranges, are due to one cause. The series is complete.

I quite agree with Mr. Darwin that a layer of water vapour would exist (and be visible) over the ice on the moon if only the temperature be high enough; but, at very low temperatures, ice practically does not vaporise even *in vacuo* (see Ganot's "Physics"). Aqueous vapour not being seen, I conclude the temperature is below (say) – 80° C. But the most potent argument in favour of my theory is that it reasonably and consistently explains *all* the peculiar features of lunar surfacing, *i.e.*:—

The absence of distinct Polar caps;
The absence of water and aqueous vapour (now);
The absence of distinct colour in details;
The brightness of all raised, rugged surfaces, mountains, cliffs, peaks;
The relative darkness of levels whereon meteoric dust can lie;
The extraordinary circularity of forms, large and small, incomplete, or overlapped;
The cones, whether central or isolated;
The clefts or rills, also strings of craterlets;
The maritime zones, ridges, and banks;
The haze or cloud, and nimbus or rayed brightness;
The dark points seen by Dr. Klein;
Lastly, if not least, the long bright rays.

I do not think I overstate the case when I say that selenographers will find these features consistently solved by the one hypothesis, and no enigmas left.

I cannot ask for space to go into details here, but will forward a short synopsis of the leading features, in case they may be required, arranging them as nearly as may be as in the preceding list.

S. E. Peal

Sibsagar, Assam, October 13

The Astronomical Theory of the Great Ice Age

The lecture and the letter of Sir Robert Ball, however lucid, do not appear to carry this question further than where Dr. Croll left it. It is easy to understand that when the shape of the earth's orbit was different, winter days might be colder and summer days hotter than now. What the theory at present wants is an exposition of the successive series of effects by which this state of climates would transform the Emerald Isle into a mere Greenland. It is scarcely an explanation to say that "vast fluctuations like these must correspond to vast climatic changes of the kind postulated." We desire to be shown that they will correspond, and that the correspondence will be of the kind required. Taking Sir Robert Ball's own illustration, I am quite ready to admit that his horse alternately starved and crammed will not run a dead heat with one uniformly fed; but in default of experience I should not feel certain that his animal would die of accumulated fat.

We know that there have been past periods of heat-supply more uniform than at present, and periods of wider fluctuation. We see also in geological records ages of vast snow accumulation and ages of rich vegetation near the Pole. We need a demonstration that such wider fluctuations do tend to the one and not to the other; towards snow-accumulation and not towards snow-dissipation. Attempts in this direction have been made, but much seems needed yet.

E. Hill

St. John's College, Cambridge, November 23

Meteor

The large meteor described in Nature by Mr. P. L. Sclater, was observed here as follows:—

Nov. 17, 7h. 18m.—Fireball many times brighter than Venus. Path from 32½° + 45° to 158° + 55°. Motion very slow, duration 7 seconds. Train, but no enduring streak. The fireball, as it gradually descended to the northern horizon, varied greatly in brilliancy, and gave a series of flashes lighting up the sky with great effect. I have occasionally seen larger fireballs, but never observed one more satisfactorily. This meteor was observed at Handsworth, Birmingham; at Crawshaw Booth, Lancashire; and at many other parts of the country. Its unusual brightness seems to have attracted wide notice.

Fireballs from Taurus are often seen at about this epoch; but that of November 17 appears to have belonged to a radiant-point in Aries.

W. F. Denning

Bristol

Freshwater Diatoms in the Bagshot Beds

Will you kindly favour me with space to ask any of your numerous readers, who may be specially interested, if they can furnish me with any references to published records of fresh-water Diatoms being observed in the carbonaceous earthy sands of the Middle and Lower Bagshot Beds of the London Basin? In conjunction with one of my pupils, I have lately subjected many of these green and dark-grey sands and earths to microscopic examination; and our labours have been rewarded by the discovery of a rather extensive unicellular flora, particulars of which will be shortly laid before the Geological Society. Meanwhile, I shall be happy to have the co-operation of other workers in the same field.

A. Irving

Wellington College, Berks, November 28

THE MATHEMATICAL TRIPOS[1]

I.

It is with the greatest pleasure that I avail myself this evening of the already well-established custom which permits one of our members, once in two years, to address to his colleagues a few general remarks connected with the science that forms our common bond of union. It is not often that a mathematician has an opportunity of laying before his fellow-workers, by word of mouth, any views of his except such as relate to the actual mathematical investigations upon which he is engaged, which, from their very nature, can appeal directly only to the few who have laboured in the same field; and I feel it to be a high privilege to be permitted, in this room, and surrounded by familiar faces, to give expression to my thoughts and hopes upon subjects that are of common interest to us all as mathematicians.

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[1] Address delivered before the London Mathematical Society by the President, Mr. J. W. L. Glaisher, M.A., F.R.S., on vacating the chai November 11, 1886.

I have not ventured to attempt any remarks upon the wide region of pure mathematics, or even upon the progress of such portions of it as have attracted the greatest share of interest among ourselves. I have felt that, as one who has resided and lectured in Cambridge for the past fifteen years, the most appropriate subjects for my address would be those upon which my residence in the University during an eventful period, or my experience as a lecturer, might to some extent qualify me to speak. Still, even when so restricted, I have found it no easy matter to decide upon the subjects to which I was most desirous of drawing your attention to-night.

I should like to have spoken at length upon the theory of elliptic functions. For fourteen years I have lectured regularly, each year, upon this subject, and no lectures of mine have been of so much interest to me. I believe that the time is rapidly approaching when the elementary portions of the theory will be regarded as necessarily forming part of the common course of reading of all students of mathematics, so that a familiarity with sn's, cn's, dn's, and their properties will become as essential as the differential calculus to the mathematical equipment of every person who has made mathematics one of his subjects of study.

Quite apart from its far-reaching influence in all branches of pure mathematics and its widespread applications in mathematical physics, there are special reasons which make the theory of elliptic functions a subject of peculiar interest in a course of mathematical studies, and one to which it is important that the student should be introduced as early as possible in his career, whether he be reading mathematics for its own sake, or for the sake of its applications, or for its advantages as a mental training. It is the first mathematical "theory" that he meets with in his reading—meaning by a "theory" a body of theorems and properties of functions so related to each other that the student cannot fail to see from the equations themselves that they form a consistent and remarkable system of facts, worthy of study on their own account, irrespective of any applications of which they may be susceptible. It is true that trigonometry, if regarded as the theory of singly periodic functions, is a theory in this sense, but it is reached by the student at too early a stage for him to be enabled to appreciate the nature and importance of facts that are expressed in the mathematical language of formulæ, and even if it were not so, the manner in which the subject is treated in text-books (the functions being derived from the circle and applied to the solution of triangles, &c., before they are considered analytically) makes it difficult to separate the mathematical theory from its various applications. In analytical geometry, which the student next meets with in his reading, a method of representing curves by equations is explained, and applied to the investigation and proof of properties of conics. In his next subject, differential calculus, he is introduced to new conceptions and processes of the very highest importance and the most fundamental character, and is taught to apply them to the investigation of maxima and minima, tangents and asymptotes to curves, envelopes, &c. Then come the elements of the integral calculus and of differential equations: the former consisting of a few chapters giving methods of integrating various classes of functions, followed by applications to curves and surfaces; and the latter of rules and methods for treating such equations as admit of finite solution.

Not one of these subjects, in the form in which they are necessarily presented to students, is an end in itself or exists for itself: they consist of ideas, methods, processes, and rules, which the student is taught to apply and to understand; they contain the conceptions with which he has to make himself as familiar as with the commonest facts of life, the tools which he is to have ever ready to his hand for use. And in the course of acquiring this knowledge he is made acquainted with numerous connected series of propositions—such as the properties of conics—besides various important results of more purely analytical interest. But all of these developments are presented to him in a form which throws no light upon the manner in which they were originally discovered, and, though the propositions are made to follow one another in clear logical order, the student cannot but be sensible that he is travelling, not along a natural highway, but upon a well-worn road, artificially constructed for his convenience. It is not till he reaches the subject of elliptic functions that he has the opportunity of seeing how, by means of the principles and processes that he has learned, a theory can be developed in which one result leads on of itself to another, in which every system of formulæ suggests ideas and inquiries about which the mind is eager to satisfy itself, and opens to the view fresh formulæ connected by unsuspected relations with others already obtained, so that he cannot resist the feeling that the subject is taking its own course, and that he is merely a bewildered spectator, delighted with the results which unfold themselves before him. He feels that the formulæ are, as it were, developing the subject of themselves, and that his part is passive: it is for him to follow where the formulæ point the way, and be amazed by the new wonders to which they lead him.

It may be that in using this language I am expressing the feelings of a mathematician, rather than those of a student on reading the elements of the subject for the first time; still I am convinced that the attributes I have just referred to are those which distinguish a genuine mathematical theory from a mere collection of useful principles and facts, and that no one can have studied elliptic functions without realising that mathematics is not only a weapon of research but a real living language—a language that can reveal wonderful and mysterious worlds of truths, of which, without its help, the mind could have gained not the least conception. It seems to me, therefore, of the highest importance that the student should be introduced to a real mathematical theory at the earliest stage at which his knowledge will permit of his deriving from it the peculiar advantages which I have mentioned. Thus only can he obtain expanded views or a true understanding of the science he is studying. Higher algebra and theory of numbers afford other conspicuous examples of the perfection that a pure mathematical theory can exhibit, but they do not lie so directly in the line of a general mathematical course of studies. Regarded from this latter point of view, elliptic functions has the additional merit of being a subject whose importance is recognised, on account of its physical applications, even by those to whom the gift of duly appreciating the wonders of pure mathematics seems to have been partially denied.

I should have liked also to have spoken at some length upon another subject that is constantly in my thoughts: I mean the pressing need of text-books upon the higher branches of mathematics. Of text-books for use in schools we have an abundance, and each month produces a fresh supply; but it is only occasionally that we have to welcome a work intended for the use of the higher University student or the mathematician. Every one of us must sometimes have felt the want of an introductory treatise that would give the reader the fundamental propositions in some branch of mathematics which exists only in memoirs and papers scattered throughout the wilderness of *Journals* and *Transactions* of Societies. We can scarcely expect to have provided for us, in many high subjects, text-books so admirable and thorough as Dr. Salmons; still I cannot refrain from expressing the hope that in the future the number of advanced mathematical treatises may not be so infinitesimal compared with the number of memoirs as at present. I could mention several subjects that are almost at a standstill, because advance is impracticable for want of avenues by which new workers can approach them. Of necessity the literature of mathematics must always be in the main a journal literature, for the audience addressed is small; but I cannot help feeling that the disproportion between the amount of exploration effected and the attempts made to render accessible the territories explored and conquered might be greater than it is. No one can realise more vividly than I do how vastly more difficult it is to write a book than a collection of memoirs, and how beset with anxieties, for any one who is at all fastidious, is the task of arranging the fundamental properties of any comparatively new subject in clear and logical form. The sustained struggle to attain clearness, exactitude, and thoroughness in the orderly development of a complicated and mutually-connected system of propositions wears out the worker more than thrice the same amount of labour devoted to new investigations with all the fascinating excitement of successes and failures, rewards and disappointments. In writing a memoir, the mathematician begins where he pleases, and confines himself to what has interested him and what he knows he has done well. In composing a book, the author has not only to marshal into order an array of theorems of various kinds, assigning to each its due place and importance, but he has—hardest task of all, perhaps—to confine his treatise within bounds, to keep it from growing to gigantic proportions as his increased study of the subject opens up to him fresh vistas. On the other hand, however, is to be considered the great service he can thus render to his favourite study: an introductory treatise on a subject not otherwise approachable by any direct route, even if it be not of the highest class, may have done far more for its advance than could have been effected by the most brilliant memoir. Time, care, and thought are essential for the preparation of any valuable treatise, and full references to the original memoirs should be always given; if these conditions have been fulfilled, the writer has deserved well of mathematical science.

I have not been able to forbear from making the few preceding remarks upon two subjects on which I have long felt strongly; but I pass now without further delay to the main subject of my address—the Mathematical Tripos. I have thought that, in view of the importance of this examination to our science, and the frequent changes that have taken place recently, this might be a subject of no ordinary interest to our members as well as to myself. Since 1872 change has succeeded change with great rapidity, and there are probably not many outside the mathematical portion of the resident body at Cambridge who are fully aware of the present mode of conducting the examination or of the further changes already sanctioned by the Senate and which take effect next June. It is, indeed, generally known that the list of wranglers, senior optimes, and junior optimes is published in June, at about the same time as many other Tripos lists, instead of by itself in January, and that the senior wrangler is displaced from his throne, and no longer owes his position to the results of the whole examination, so that he is not necessarily—even from an examination standpoint—the first mathematician of his year. So much only is generally known; and it has seemed to me that it might be of interest, considering the influence for good that it is hoped the examination in its new form will have upon the progress of mathematics, to give some account of the successive developments that have taken place in this time-honoured examination, and the causes and efforts that have led to them. The difficulties connected with the placing of all the mathematical candidates of the year in one order of merit, the extension or limitation of the subjects of examination, and various other questions connected with the Tripos, are matters that have been continually discussed and re-discussed in the light of fresh experience by those concerned with the mathematical course of studies at Cambridge, but I may, nevertheless, perhaps be permitted to-night very briefly to refer to some of the familiar arguments in the presence of a more extended audience of mathematicians.

It is convenient to preface the principal remarks I have to make by an outline of the history of the Tripos. In doing so, I must pass very lightly over its origin and early development, as anything approaching to a complete history of its origin and rise in the last century would amount almost to a history of the studies of the University.

At the beginning of the last century, besides certain merely formal disputations, the only exercises required from candidates for degrees were the keeping of acts and opponencies. Each candidate for honours in the course of his third year had to maintain publicly a thesis, the subject of which was chosen by himself, against three opponents, in the presence of one of the Moderators, who acted as umpire. The subjects selected were philosophical or mathematical; the discussion took place in Latin and in logical form. After hearing the discussions, the Proctors and Moderators prepared a final list of candidates qualified to receive degrees. This can scarcely be considered to have been an order of merit, for each of the Proctors and Moderators, and also the Vice-Chancellor, had the right to introduce the name of one candidate into the list whenever he pleased; still, except in the case of the recipients of these honorary degrees, it is probable that the list in the main fairly represented the merits of the candidates. It was divided into three classes, consisting of (1) the wranglers and senior optimes; (2) the senior optimes who had done fairly well but had not distinguished themselves; and (3) οἱ πολλοί, or the poll-men. The first class received their degrees on Ash Wednesday, taking seniority according to their order on the list, and the two other classes received their degrees later.

With regard to the origin of the Tripos, Mr. W. W. Rouse Ball, in his interesting sketch of its history,[1] writes:—

"The impressions gathered from these disputations in the schools were necessarily rather vague, and when they became the sole University exercise for a degree they hardly afforded a sufficient basis for an accurate arrangement of the men in order of merit. It was, I believe, to correct this fault that the Senate House examination was introduced, and I am inclined to think that it had its origin about the year 1730. At first it probably consisted only of a few *vivâ voce* questions addressed by the Proctors and Moderators in the week after the schools to those candidates about whose abilities and position some doubt was felt; but its advantages were so patent that within ten or twelve years it had become systematised into a regular examination to which all questionists were liable, although technically it was still regarded as only supplementary to the exercises in the schools. From the beginning it was conducted in English, and accurate lists were made of the order of merit of the candidates; two advantages to which, I think, its final and definite establishment must be largely attributed."

Mr. Ball divides the time during which the exercises in the schools and the Tripos were concurrent into five periods: (1) from 1730 to about 1750, during which time it was probably unauthorised and regarded as an experiment; (2) from 1750 to 1763, during which it was gradually establishing itself,—in the last year of this period it was officially decided that when a candidate's position in the class-list was doubtful the Senate House examination and not the disputation was to be taken as the final test; (3) from 1763 to 1779, during which definite rules were framed and laid down for conducting it; (4) from 1779 to 1827, during which it practically superseded the disputations; (5) from 1827 to 1841, the year in which the disputations were abolished.

The lists published in the Cambridge University Calendars begin with the year 1747, because in that year the final lists were first printed and distributed, the names of those who had received honorary degrees being specially marked, so that by simply erasing them the true order of merit of the other candidates could be obtained. The division of the first class into wranglers and senior optimes was first made in 1753.

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[1] "The Origin and History of the Mathematical Tripos," Cambridge, 1880. (Reprinted from the *Cambridge Review*.)

It was in the third of the above periods, that is, between 1763 and 1779, that the Senate House examination was gradually gaining ground upon the schools in determining a candidate's final place on the list. By means of their acts and opponencies the candidates were divided by the Moderators into eight classes, each class being arranged in alphabetical order; their subsequent position in the class was then determined by the Senate House examination. The first two classes comprised those who were expected to be wranglers, the next four included the other candidates for honours, and the last two consisted of poll-men only. The classes were examined separately and *vivâ voce*. During this period it became the custom to require written answers to the questions. The examiner gave out the questions to the class one by one, giving out a fresh question as soon as he saw that any one had finished the last. The problem papers, which were confined to the first two classes, were given to the candidates in writing, so that they had the whole paper before them at once.

It may be of interest to give a more detailed account of the exercises in the schools during this period, when both the exercises and the examination were in full operation and vigour. The Moderators, having received from the tutors of the Colleges a list of the students who were candidates for honours at the next examination, fixed a day in the Lent term on which each was to keep his act, and assigned to him three opponents. The Respondent, or "Act" as he was then called, selected three subjects which he proposed to maintain, and submitted them to the Moderator, who communicated them to his three opponents, designating them *opponentium primus*, *secundus*, or *tertius*. On the day fixed for the Act the respondent read his thesis in the schools in the presence of the Moderator. The first opponent then mounted the box opposite to that of the respondent and below that of the Moderator, and joined issue with him, opposing the thesis by eight arguments of syllogistic form. The respondent replied to each in turn, and when an argument had been disposed of, the Moderator called for the next in the words *Probes aliter*. When the disputation had continued long enough, the Moderator dismissed the opponent with such words as "*Bene disputasti*," or "*Optime disputasti*," or "*Optime quidem disputasti*," as the case might be. The second and third opponents (who had to oppose the thesis by five and three arguments respectively) entered the box successively, and after disputing were dismissed in the same manner, the whole performance lasting between one hour and two hours. The respondent himself was dismissed with some such phrase as "*Satis et optime quidem tuo officio functus es*." Such compliments gave rise to the classification into senior and junior optimes. In general, "*Optime quidem*" was the highest praise expected even by future wranglers. The distinguished men of the year appeared eight times in the schools, twice as Respondents and twice in each grade of opponency.[1]

The final establishment of the Mathematical Tripos dates, as remarked by Mr. Ball, from 1779. By the regulations agreed to by the Senate in that year, the Moderators of the previous year were added to the regular staff of examiners, and the system of brackets was introduced. The examination lasted three days (the last of which was devoted to moral philosophy), and on the fourth day a class-list, called "the Brackets," was issued, in which those candidates who were nearly equal were bracketed together. One day was devoted to the "examination of the brackets," by the result of which the names in each bracket were placed in order of merit. There was also a power of challenging, by which a candidate who was dissatisfied with his bracket might challenge any other candidate he pleased to a fresh examination;[1] but it seldom happened that any one rose above or fell below his bracket. From 1779 onwards the examination slowly and surely grew in importance, and the exercises became of less account each year, till they were finally discontinued by the Moderators in 1839. Two years later they were formally abolished by the Senate.

The following account of the Senate House examination in 1802 is abridged from the Cambridge University Calendar of that year:—"On the Monday morning, a little before eight o'clock, the students, generally about a hundred, enter the Senate House, preceded by a Master of Arts, who on this occasion is styled the father of the College to which he belongs. On two pillars at the entrance of the Senate House are hung the Classes [*i.e.* the eight classes into which the candidates have been divided by the exercises in the schools; and a paper denoting the hours of examination of those who are thought most competent to contend for Honours.

"Immediately after the University clock has struck eight, the names are called over, and the absentees being marked, are subject to certain fines. The classes to be examined are called out, and proceed to their appointed tables, where they find pens, ink, and paper provided in great abundance. In this manner, with the utmost order and regularity, two-thirds of the young men are set to work within less than five minutes after the clock has struck eight. There are three chief tables, at which six examiners preside. At the first, the senior Moderator of the present year and the junior Moderator of the preceding year. At the second, the junior Moderator of the present and the senior Moderator of the preceding year. At the third, two Moderators of the year previous to the two last, or two examiners appointed by the Senate. The two first tables are chiefly allotted to the six first classes; the third or largest to the *οἱ πολλοί*." After describing the manner of reading out the questions, the account proceeds:—"The examiners are not seated, but keep moving round the tables, both to judge how matters proceed and to deliver their questions at proper intervals. The examination, which embraces arithmetic, algebra, fluxions, the doctrine of infinitesimals and increments, geometry, trigonometry, mechanics, hydrostatics, optics, and astronomy, in all their various gradations, is varied according to circumstances: no one can anticipate a question, for in the course of five minutes he may be dragged from Euclid to Newton, from the humble arithmetic of Bonnycastle to the abstruse analytics of Waring. While this examination is proceeding at the three tables between the hours of eight and nine, printed problems are delivered to each person of the first and second classes; these he takes with him to any window he pleases, where there are pens, ink, and paper prepared for his operations." At nine o'clock the papers had to be given up, and half an hour was allowed for breakfast. At 9.30 all returned and were examined in the same way till eleven, when the Senate House was again cleared. An interval of two hours then took place. At one o'clock all returned again and were examined. At three o'clock the Senate House was again cleared for half an hour, and on the return of the candidates the examination was continued till five o'clock. This ended the Senate House examination for the day, but at seven in the evening the first four classes went to the senior Moderator's rooms to solve problems. They were finally dismissed for the day at nine, after eight hours of examination.[1] The work on the next day (Tuesday) was similar to that of the Monday; Wednesday was devoted to logic, moral philosophy, &c. "On Thursday the examinations are resumed, and continued nearly as usual, as on the Monday and Tuesday. At eight o'clock the new classifications, or brackets, which are arranged according to the order of merit, each containing the names of the candidates placed alphabetically, are hung upon the pillars." Fresh editions and revisions of the brackets were published at 9 and 11 a.m. and at 3 and 5 p.m., according to the course of the examination, liberty being given to any man to challenge the bracket immediately above his own. At five the whole examination ended. The Proctors, Moderators, and examiners then retired to a room under the public library to prepare the list of Honours, which was sometimes settled without much difficulty in a few hours, but sometimes not before two or three the next morning. The name of the senior wrangler was generally published at midnight. In 1802 there were eighty-six candidates for honours, and they were divided into fifteen brackets, the first and second brackets containing each one name only, and the third bracket four names.

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[1] Wordsworth, *Scholæ Academicæ* (1877), p. 37. A specimen of an argument, expressed in scholastic form, on the question, "Recte statuit Paleius de Virtute," is given by Wordsworth on p. 39. and the full system of eight arguments (in a disputation of 1784) on the question, "Solis parallaxis ope Veneris intra solem conspiciendæ a methodo Halleii recte determinari potest," is reproduced in detail by Mr. Ball in the appendix to the sketch already referred to. In the latter part of the last century it seems to have been usual for two of the questions to relate to mathematics and the third to moral philosophy. Wordsworth mentions that in 1710-11 it needed all the influence of an enthusiastic Proctor and Moderator to induce a student to keep his act in mathematical questions, but that by the middle of the century the examination was so far crystallising into the *Mathematical* Tripos that a questionist was enabled by academical authority in 1750 to resist the demands of a Moderator to produce one metaphysical question, he having already distinguished himself in mathematical argument. In the early Cambridge University Calendars the three questions given as specimens are: (1) "Recte statuit Newtonus in septima sua sectione Libri primi"; (2) "Iridis primariæ et secundariæ Phenomena solvi possunt ex Principiis Opticis"; (3) "Recte statuit Lockius de Qualitatibus Corporum."

[1] In such cases the Moderators called to their assistance the Proctors or other Masters of Arts. About 1770 any Master of Arts was at liberty to examine any of the candidates. Mr. Ball is of opinion that this right was not insisted on after 1785.

The examination seems to have been a time of great excitement, and the list was most anxiously awaited. This was the case also before 1779, as appears from the account of the contest between Paley and Frere for the senior wranglership in 1763 and Jebb's account of the examination in 1772.[2]

Various changes took place in the examination in 1808, 1828, 1833, and 1839. In 1808 another day was added: three days were devoted to mathematics, exclusive of the day of examination of the brackets. Each candidate was employed eighteen hours in the course of the three days, of which eleven were devoted to book-work and the remaining seven to problems. By regulations which were confirmed by the Senate on November 13, 1827, and came into operation in January 1828, another day was added, so that the examination extended over four days, exclusive of the day of examining the brackets; the number of hours of examination was twenty-three, the time assigned to problems being the same as in 1808. On the first two days all the candidates had the same questions proposed to them, inclusive of the evening problems, and the examination on those days excluded the higher and more difficult parts of mathematics, in order, in the words of the report, "that the candidates for Honours may not be induced to pursue the more abstruse and profound mathematics to the neglect of more elementary knowledge." Accordingly, only such questions as could be solved without the aid of the differential calculus were set on the first day, and those set on the second day involved only its elementary applications. The classes were reduced to four, determined as before by the exercises in the schools. The regulations of 1827 are specially important, because they first prescribed that all the papers should be printed.[3] They are also noticeable as being the last which gave the examiners power to ask *vivâ voce* questions: after recommending that there be not contained in any paper more questions than well-prepared students have been generally found able to answer within the time allowed for the paper, the Report proceeds: "But if any candidate shall, before the end of the time, have answered all the questions in the paper, the examiners may at their discretion propose additional questions *vivâ voce*."

New regulations were confirmed by the Senate on April 6, 1832, and took effect in 1833. The commencement of the examination was placed a day earlier, the duration was five days, and the hours of examination on each day were five and a half. Thus four and a half hours were added to the whole time of examination, four of which were assigned to book-work and the remaining half-hour to problems. The examination on the first day was confined to subjects that did not require the differential calculus, and on the second and third days only the simple applications of the calculus were included. During the first four days of the examination the papers were set to all the candidates alike, but on the fifth day the examination was conducted according to classes. No reference is made to *vivâ voce* questions, and the examination of the brackets only survives in the permissive form: "That the result of the examination be published in the Senate House on the morning of the Friday succeeding the first Monday in Lent term, at nine o'clock; but if it should happen that the relative merits of any of the candidates are not then determined to the satisfaction of the Moderators and Examiners, that such candidates be re-examined on that day."

Only six years later these regulations were superseded by a new system, which passed the Senate on June 2, 1838, and came into operation in January 1839. By these new regulations another day was added to the examination, which thus lasted six days. The total number of hours of examination was thirty-three, of which eight and a half were given to problems. Throughout the whole examination the same papers were set to all the candidates. The regulations contain no mention of classes, and accordingly the exercises in the schools were discontinued by the Moderators. The permissive rule relating to the re-examination of the candidates (the survival from the brackets) was retained in these regulations in the same form as in those of 1832.

It is very interesting to notice, in the successive regulations, how the *vivâ voce* examination gradually merged into an examination by printed papers, and how, as the examination became more elaborate and exacting, it rendered unnecessary, not only the preliminary exercises in the schools, but also the final examination of the brackets.

Besides the development of the Senate House examination itself, other changes had taken place in the University system during this period of renewed activity. In 1824 the first Classical Tripos examination took place; only those who had already passed the mathematical examination being admissible as candidates. The name "Mathematical Tripos"[1] dates from after this year.

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[1] In 1818 the hours for the evening problem paper were 6-10, so that the candidates had ten hours' examination in the day. Originally, as mentioned above, the problems were only set to the first two classes; in 1802 they were open to four classes, and in 1818 to all six classes, *i.e.* to all the candidates for Honours.

[2] Wordsworth (pp. 47 *et seq.*). See also the letters of Gooch, who was second wrangler in 1771 (p. 322).

[3] The words of the report are:—"It is further recommended that the questions from books, which have hitherto been proposed to the classes *vivâ voce*, should, for the future, be printed. And it is hoped that, as by this means the questions proposed in each year will be more generally known, the students may thus be better directed in their reading than they now are, and the mathematical studies of the University become more fixed and definite. An opportunity will also be afforded of ascertaining by an inspection of these papers that the examination embraces a due proportion of each of the ordinary subjects of mathematical study.

"As, however, in proposing this alteration, the intention of the Syndicate is to avoid making any change in the substance of the examination, it is recommended that the examiners be strictly enjoined to insert in these papers such questions only as are at present proposed *vivâ voce*; namely, propositions contained in the mathematical works commonly in use in the University, or simple examples and explanations of such propositions."

[1] There are few matters in the history of the University more curious and interesting than the manner in which the word *Tripos* came to be applied to the Senate House examination, and consequently also to the other final Honour examinations. It is natural to suppose that it is connected with the three classes into which the Honour list was divided, but there is no connection whatever. The history of the name may be given briefly as follows:—In the ceremonies which were performed on Ash Wednesday in the middle of the sixteenth century, at the admission of questionists to be Bachelors of Arts, an important function was executed by a certain "ould bachilour," who was appointed as champion on the side of the University. He had to "sit upon

Previously it had been known as the Senate House Examination, and this name continued long afterwards and for more than thirty years was still printed as a heading to the papers set. It was only as a means of distinguishing it from the Classical and other newer Triposes that the name Mathematical Tripos gradually came into use. By the regulations which took effect in 1828 a totally distinct series of papers were set to the poll-men, who then formed the fifth and sixth classes. The fiction of regarding the poll-list as a continuation of the list of mathematical honours still lingered till 1858, the names being arranged in order of merit in four classes called the fourth, fifth, sixth, and seventh; the fourth class being in theory supposed to be the next class to that of the junior optimes.

(*To be continued.*)

THE COLOURS OF METALS AND ALLOYS[1]

THIS lecture is published by request, but the author fears that, dealing as it does with the colours of metals, such interest as it may have possessed when delivered will be greatly diminished in the absence of the experiments and diagrams by which it was illustrated.

I begin with no ordinary pleasure the work which has been intrusted to me by the Council of the British Association. It is nearly twenty years since this series of lectures was established. The first, on "Matter and Force," was delivered at Dundee by a brilliant experimenter and one of the most eloquent living exponents of science; it was followed, at Norwich, by a lecture by Prof. Huxley, to whom the nation owes a deep debt of gratitude for his intense sympathy with all who are seeking to widen the bounds of scientific knowledge– to be whose colleague in one of the most important scientific schools of the country is my great good fortune. These lecturers were succeeded by other eminent men, among whom may be mentioned Spottiswoode, Bramwell, and Lubbock. The object of the lectures is to diffuse a knowledge of the operations of Nature, and to add to the number of those who rejoice in her works. Many, therefore, who have spoken to audiences similar to this, have appealed to natural phenomena; and instead of talking to you about the colour of metals, I also should have liked to dwell on the colour of the sea and sky, but Ruskin's works are, I know, often in your hands, and he has told you once for all of the colour of clouds that "there is not a moment of any day of our lives when Nature is not producing scene after scene, picture after picture, glory after glory, and working still upon such exquisite and constant principles of the most perfect beauty that it is quite certain it is all done for us, and intended for our perpetual pleasure."[1] The metallurgist, however, cannot speak with authority on themes such as these; and I have therefore selected a subject which will, I believe, enable me to bring before you important truths affecting a wide range of industrial operations, and at the same time to sustain the true function of art by pointing to the direction in which we may have increased pleasure in things that constitute our most ordinary possessions, and which we use in daily life. First permit me to explain that I intend to include under the title of the lecture any facts which are, in my opinion, connected with the colours of metals and alloys, whether natural to them or produced by artifice, as well as a brief examination of the influence which the colours of metals appear to have exerted on the history of science.

I propose to begin at what will appear to be a somewhat remote starting-point. We say that copper is red, gold yellow, and silver white, but it is by no means certain that the early races of the world had any very clear perception of the difference between these several metallic colours. With regard to early Hebrew and Greek civilisation, Mr. Gladstone has expressed his belief that the colour-sense—that is the power of recognising colour and distinguishing it from mere brightness or darkness—was imperfectly developed, and he considers that "the starting-point is absolute blindness to colour in the primitive man," and he urges that the sense of colour has been gradually developed "until it has now become a familiar and unquestioned part of our inheritance." He adds: "Perhaps one of the most significant relics of the older state of things is to be found in the preference (known to the manufacturing world) of the uncivilised nations for strong and, what is called in the spontaneous poetry of trading phrases, loud colour."[2]

Dr. Magnus holds the view that the colour-sense in man has undergone a great improvement within the last 2000 years, and Prof. Haeckel supports this speculation, but it is opposed by Romanes, who has favoured me with some observations on the subject, in view of this lecture; and it seems to me strange, if savage nations are really deficient in the sense of colour, that the use of such colours as they can get in metals and fabrics, blended, for instance, in a war-club or a pipe-stem, should be so thoroughly "understood" and so discriminatingly employed as we sometimes find them to be. It may further be observed that primitive man may even have derived from his more remote ancestry some power of being influenced by colour, and we are told that the attraction which gorgeous colouring in flowers has for birds and insects, and which colour generally possesses for our nearer ancestors, has been of great importance in the origin of species, and in the maintenance of organic life.

No doubt, in ancient times, there was much confusion between mere brightness and colour, such as is evident in the beautiful sentence in which St. Augustine[3] says: "For this *queen of colours*, *the light*, bathing all which we behold, wherever I am through the day, gliding by me in varied forms, soothes me when engaged on other things and not observing her." If, however, it were proved that the power of distinguishing the colour of metals was not widely diffused among the Egyptians, Hebrews, and Greeks, it is at least certain that there were individuals of these nations to whom, in very early times, the colour of metals was all-important; and although they may have confused different precious stones under generic names, they certainly appreciated their various colours, and knew, moreover, that by fusing sand with the addition of a small quantity of certain minerals, they could produce artificial gems of varied tints.

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a stoole before Mr. Proctours" and to dispute with the "eldest son" (the foremost of the questionists), and afterwards with "the father" (a graduate of the College to which the "eldest son" belonged, representing the paternal character of the College). At this time the only "Tripos" was the three-legged stool on which the Bachelor sat. A century later this Bachelor seems to have become a sort of licensed buffoon, and to have been called "Mr. Tripos," just as a president is sometimes referred to as "the Chair," or a judge as "the Bench." During the 120 years in which the name Tripos or Tripus indicated a personage there are frequent allusions to the humorous orations delivered in the schools by those who filled this office. These were known as Tripos speeches. It is probable that Mr. Tripos ceased to take part in the arguments in the schools between 1730 and 1750, just about the time when the Senate House examination was originating. The Tripos speeches were then replaced by copies of Latin verses, which were circulated on the admission days. These were called Tripos verses. About 1747 the Moderators began the custom of printing Honour lists on the back of the Tripos verses. Thus the list of Honours in the Senate House examination came to be called the Tripos list, so that a man's name was said to stand in such a place in the Tripos of his year, *i.e.* upon the back of the Tripos verses. And, lastly, the name was transferred from the list to the examination, the results of which were published in the list. This account is abridged from Wordsworth's *Scholæ Academicæ*, chap. ii. Wordsworth concludes: "Thus, step by step, we have traced the word *Tripos*, passing in signification, Proteus-like, from a thing of wood (*olim truncus*) to a man, from a man to a speech, from a speech to two sets of verses, from verses to a sheet of coarse foolscap paper, from a paper to a list of names, and from a list of names to a system of examination."

[1] A Lecture delivered on September 3 by Prof. W. Chandler Roberts-Austen, F.R.S., to the Operative Classes in the Town Hall of Birmingham, in connection with the meeting of the British Association.

[1] "Modern Painters," vol. i. part 2, p. 201, 1851.

[2] *Nineteenth Century*, p. 367, 1877.

[3] "Confessions of St. Augustine." Edition edited by E. B Pusey, D.D. (p. 211).

My object in leading you so far back—in discussing what appears to be a very matter-of-fact subject—is to point to the close connection between the early recognition and appreciation of colour in metals or minerals, and the foundation of the science of chemistry.

In early scientific history the seven metals known to the ancients were supposed to be specially connected with the seven principal planets whose names they originally bore, and whose colours were reflected in the metals; thus gold resembled the sun, silver the moon, while copper borrowed its red tint from the ruddy planet Mars. The belief in the intimate relation between colours and metals, the occult nature of which they shared, was very persistent, and we find a seventeenth-century writer, Sir John Pettus, saying[1] that "painters" derive "their best and most proper colours from metals whereof seven are accounted the chief, produced from the seven chief metals, which are influenced by the seven planets." A survival of this feeling is suggested by a modern writer, Leslie, who supposed that "when Newton attempted to reckon up the rays of light decomposed by the prism, and ventured to assign to them the famous number seven, he was apparently influenced by some lurking disposition towards mysticism."[2]

It would be impossible for me to overrate the importance of the colour of metals in relation to scientific history, for the attempt to produce a metal with the colour and properties of gold involved the most intense devotion to arduous research sustained by feverish, hope, attended by self-deception and elaborate fraud, such as hardly any other object of human desire has developed. It led to despair, to madness, and to death; but finally, through all, alchemy prepared the way for the birth of chemistry, and for the true advancement of science.

In early times, as now, gold was an extremely desirable form of portable property, and its colour was, perhaps, held to be the most distinctive and remarkable fact about it. I may incidentally observe that the dominant idea of colour in connection with the metallic currency survives in the familiar phrase, "I should like to see the *colour* of his money," which curiously expresses a desire, tempered by doubt as to its fulfilment. On looking back, we find that, at least from the third to the seventeenth century, the colour of gold haunted the early experimenters, and induced them to make the strangest sacrifices, even of life itself, in the attempt to imitate, and even actually to produce, the precious metal. Let us see what kind of facts were known within the period I have indicated. In barbaric times, hammered pieces of gold, or gold beaten into thin sheets and plates, were used with coloured stones and coral for personal adornment. The next step was to make gold go further by gilding base metals with it, and, in order to do this, the colour was for the moment sacrificed by combining the gold with quicksilver. This was done at least in the time of Vitruvius, B.C. 80, heat being used to drive away, as vapour, the quicksilver which had been united to the gold, leaving a thin film of precious metal on the surface to be gilded. But this was possibly not the first method of gilding, for we now know, from a papyrus of about the third century[3] of our era, that lead was used for this purpose. Gold, when fused with lead, entirely loses its golden colour, and yet, by the application of heat in air, the lead may be made to flow away as a fusible oxide, leaving the precious metal on the metallic object to be gilt, the base metal being as it were transmuted, superficially at least, into gold. The point I want to insist upon is that the metallic colour of the gold vanished during the process as carried on by the craftsman, only to re-appear at the end of the operation; and I am satisfied that it was from such simple technical work as this that the early chemists were led to think that the actual production of gold—the transformation of base metals into gold—was possible. The more observant of them, from Geber, the great Arabian chemist of the seventh century, to our own countryman, Roger Bacon, in the thirteenth, saw how minute a quantity of certain substances would destroy the red colour of copper, or the yellow colour of gold. A trace of arsenic will cause the red colour of copper to disappear; therefore, the alchemists very generally argued, some small quantity of the right agent, if only they could find it, will turn a base metal to the colour of gold. Look, they said, how small a quantity of quicksilver will change the appearance of metallic tin. Here is a bar of tin 2 feet long and 1 inch thick, which it would be most difficult to break, though it will readily bend double. If only I rub a little quicksilver on its surface a remarkable effect will be produced, the fluid metal will penetrate the solid one,[1] and in a few seconds the bar will, as you see, break readily, the fractured surface being white, like silver. It was by such facts as this that men were led to believe that the white metal, silver, could be made.

Successive workers at different periods held divergent views as to the efficacy of the transmuting agent. Roger Bacon, in the thirteenth century, held that one part of the precious substance would suffice to turn a million parts of base metal into gold. Basil Valentine, in the fourteenth century, would have been content with the transmutation of seventy parts of base metal by one part of the agent. While, coming to the end of the eighteenth century, Dr. J. Price, F.R.S., of Guildford, only claimed that the substance he possessed would transmute from thirty to sixty parts of base metal.[2]

It is a curious fact that no one seems to have actually prepared the transmuting agent for himself, but to have received it in a mysterious way from "a stranger"; but I must not dwell on this. I will merely point out how persistent was the view as to the singular efficacy of the transmuting agent, and I will content myself with a reference to Robert Boyle, our great countryman, an accurate chemist of the seventeenth century, who did more than any one else to refute the errors of alchemy. He nevertheless characteristically records[3] the following experiment, in which, instead of ennobling a base metal, he apparently degraded gold to a base one. He first purified a small quantity of gold, about "two drachms," with great care, and, he states, "I put to it a small quantity of powder communicated to me by a stranger," —it is singular that even he should have received the transmuting agent in the usual way,—"and," he adds, "continuing the metal a quarter of an hour on the fire, that the powder might diffuse itself through it, . . . the metal when cold appeared to be a lump of *dirty colour;* . . . 'twas brittle, and, being worked with a hammer, it flew into several pieces. From hence," he adds, "it appears that an operation almost as strange as that called 'projection'" (or transmutation) "may safely be admitted, since this experiment shows that gold, . . . the least mutable of metals, may in a short time be exceedingly changed . . . by so small a portion of matter that the powder transmuted a thousand times its weight of gold." He elsewhere observes of a similar experiment, "transmutation is nevertheless real for not being gainful, and it is no small matter to remove the bounds which Nature seems very industriously to have set to the alterations of bodies."[4] The change in the colour of the gold was remarkable, but Boyle had only produced one of the series of alloys most dreaded by every jeweller—"brittle gold"—for the way in which an alloy of gold and copper is affected by a small quantity of impurity presents one of the most serious difficulties in working gold. It has been known since the seventh century, that minute quantities of certain metals render gold brittle, and it may be well to demonstrate the fact.

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[1] "Fleta Minor," 1686, Appendix, "Essay on Metallic Words.—Colour."

[2] "Treatises on Various Subjects of Natural and Chemical Philosophy."

[3] "Les Origines de l'Alchimie," par M. Berthelot, 1885, pp. 82, 89. It is interesting to compare the account of this method of gilding by lead with the expression used by Homer, who says: "As when gold is *fused* around the silver by an experienced man."—"Odyssey," vi. 232-35, quoted by Schliemann, "Ilios," p. 258, in relation to a gilded knife of copper which he permitted me to analyse in 1878.

[1] Homberg, *Mém. de l'Acad. Royale des Sciences*, 1713 (vol. published 1739), p. 306.

[2] "An Account of some Experiments on Mercury, Silver, and Gold made at Guildford, in the Laboratory of James Price, M.D., F.R.S.," Oxford, 1782.

[3] "The Philosophical Works of the Hon. Robert Boyle" (Shaw's second edition), 1738, vol. i. p. 78.

[4] *Ibid.* p. 262.

Here are two hundred sovereigns: I will melt them and will add in the form of a tiny shot a minute portion of lead amounting to only the 2000th part of the mass, first, however, pouring a little of the gold into a small ingot, which we can bend and flatten, thus proving to you that it is perfectly soft, ductile, and workable. The rest of the mass we will pour into a bar, and now that it is sufficiently cold to handle, you see that I am able to break it with my fingers, or at least with a light tap of a hammer. The *colour* of the gold is quite altered, and has become orange-brown, and experiments have shown that the tenacity of the metal, that is, the resistance of the gold to being pulled asunder, has been reduced from 18 tons per square inch to only 5 tons. These essential changes in the property of the metal have been produced by the addition of a minute quantity of lead. I have cited these facts mainly to show that the changes produced in the colour and properties of metals by small variations of composition were such as to lead the alchemists on in their belief that it was possible to change lead or tin into gold, and the hope in which they worked enabled them to gather facts out of which chemical science was gradually constructed. We shall see presently that changes in the colour of metals and alloys produced by the addition of small quantities of foreign matter, are of great importance in the application of metals to artistic purposes, but we must first try to examine more closely some of the prominent facts connected with the colour of metals, that is, the effect metals have on light so as to produce the effect of colour in our eyes. We are apt to think of gold as being essentially and distinctively golden-yellow; it may, however, possess a wide range of colours without in any way losing the condition of absolute metallic purity, its relations to light depending entirely on the nature of its surface, and especially on whether the metal is in mass or in a more or less fine state of division. Interesting as gold is to us in mass (and I may incidentally mention that during my official connection with the Mint I have been responsible for the quality of 462 tons of it) it is perhaps still more interesting to us when beaten so fine that a single grain, of the value of 2*d*., would cover a space of 48 square inches, or when it is so finely divided that the dimensions of a single particle may closely approximate to those of the ultimate atom.

This aspect of the question was investigated by Faraday, and the experimental part of the subject remains practically unadded to since his time. It is well known that a leaf of gold when seen by transmitted light is either green or blue, according to its thickness. Here is such a leaf of green gold, as seen when light is actually sent through it (Fig. 1), so as to project a green disk on the screen. A portion of the light will be *reflected* from its surface, and this reflected ray may be caught in a mirror and thrown on the screen so that you have, shown side by side, the green disk of transmitted light and the golden one of reflected light from the same leaf of gold.

Gold may readily be converted into a soluble chloride which produces a beautiful golden solution. If such a solution contains very little gold, not more than 2 grains in a gallon, and if certain chemical methods be adopted to precipitate the gold, that is, to throw it out of solution in a *solid*, though in a very fine state of division, the metal may exhibit a wide range of tint, from ruby to black.

[A few drops of phosphorus dissolved in bisulphide of carbon had been added to about a gallon of a very dilute solution of chloride of gold contained in a tall glass cylinder, as shown in the sketch (Fig. 2). The beam from an electric light, thrown through the vessel, revealed in the lower part the presence of finely-divided metal of the natural golden colour, while the more finely-divided gold in suspension imparted a brilliant ruby colour to the liquid, and a glowing ruby disk was projected on a white screen.]

When gold is in the "ruby" state, it is so finely divided that each particle probably approximates to the dimensions of the gold atom.

[The solar spectrum was then thrown upon the screen, and the audience was invited to compare it with a diagram which, while closely resembling the solar spectrum, really represented the way in which pure metallic gold, prepared by various methods, is capable of behaving in relation to light so as to produce the sensation of a wide range of colours.]

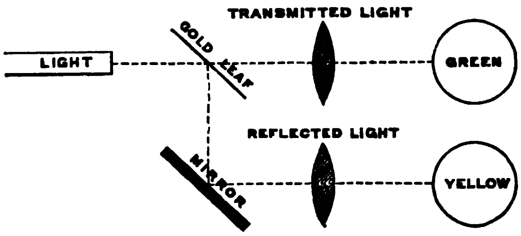

FIG. 1.

It would be easy to show that light is similarly affected by other metals; but I have selected gold for the purpose of illustration because it is easy to maintain it in a state of purity, however finely divided it may be. We must therefore modify any views we may have formed as to a metal having exclusively a special colour of its own, because it will be evident that a particular colour is only due to a definite state of arrangement of its particles. The intimate relation between the state of the surface of a metal and its colour is well shown by the beautiful buttons devised by Sir John Barton. He proved that if very fine lines be drawn close together, so that about 2000 would be ruled in the space of an inch, a beautiful iridescent effect is produced, the tints being quite independent of the metal itself due to an optical effect of the lines.

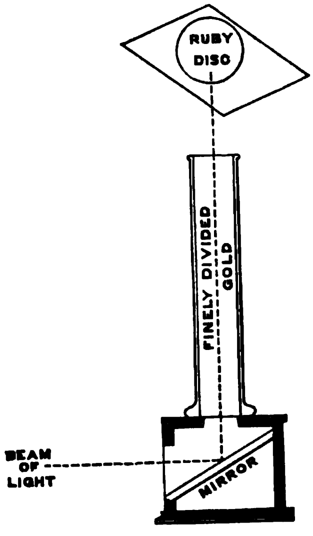

FIG. 2.

[The image of such a button was then thrown upon the screen]

Let us now examine some effects of uniting metals by fusing them together into what are called alloys; and, second, the direct influence of a minute quantity of one metal in changing the mass of another in which it is hidden, and causing it to behave in a different way in relation to light, and consequently to possess a colour different from that which is natural to it; or the added metal may so change the chemical nature of the metallic mass that varied effects of colour may be produced by the chemical combinations which result from the action of certain "pickling" solutions. This portion of the subject is so large that I can only hope to set before you certain prominent facts.[1]

First, with reference to the colour produced by the union of metals. Here is a mass of red copper, and here one of gray antimony: the union of the two by fusion produces a beautiful violet alloy when the proportions are so arranged that there is 51 per cent. of copper and 49 per cent. of antimony in the mixture. This alloy was well known to the early chemists, but unfortunately it is brittle and difficult to work, so that its beautiful colour can hardly be utilised in art. The addition of a small quantity of tin to copper hardens it, and converts it, from a physical and mechanical point of view, into a different metal. The addition of zinc and a certain amount of lead to tin and copper confers upon the mettal copper the property of receiving, when exposed to the atmosphere, varying shades of deep velvety brown, characteristic of the bronze which has from remote antiquity been used for artistic purposes. But by far the most interesting copper alloys, from the point of view of colour, are those produced by its union with zinc, namely brass. Their preparation demands much care in the selection of the materials, and I might have borrowed from the manufacture of brass instance after instance of the influence of traces of impurity in affecting the properties of the alloy, but it is unnecessary to dwell upon this alloy in Birmingham, for in all that relates to the mechanical manipulation of the alloys of copper with tin and with zinc, you are masters. I have many inducements in this place to speak about this beautiful alloy. I am proud to be a namesake of the craftsman, William Austen, who, in 1460, made that magnificent monument in brass which covers the remains and commemorates the greatness of Richard Beauchamp, Earl of Warwick, and I am glad to remember that Queen Elizabeth granted the first patent for the manufacture of brass in England to William Humfrey, Assay Master of the Mint, a predecessor in the office it is my privilege to hold.

I want, however, to direct your attention to-night to some alloys of copper with which you are probably less familiar than with brass. In this direction Japanese art affords a richer source of information than any other. Of the very varied series of alloys the Japanese employ for art metal-work, the following may be considered to be the most important and typical. The first is called "shaku-dō"; it contains, as you will observe from Analyses I. and II.,[2] in addition to about 95 per cent. of copper, as much as 4 per cent. of gold. It has been used for very large works. Colossal statues are made of it; one cast at Nara in the seventh century being specially remarkable. The quantity of gold is, however, very variable; specimens I have analysed contained only 1·5 per cent. of the precious metal. The next alloy to which I would direct your attention is called "shibu-ichi." There are numerous varieties of it, but in both these alloys, shaku-dō and shibu-ichi, the point of interest is that the precious metals are, as it were, sacrificed in order to produce definite results; gold and silver, when used pure, being employed very sparingly to heighten the general effect. In the case of the shaku-dō, we shall see presently the gold appears to enable the metal to receive a beautiful rich purple coat or *patina*, as it is called, when treated with certain pickling solutions; while shibu-ichi possesses a peculiar silver-gray tint of its own, which, under ordinary atmospheric influences, becomes very beautiful, and to which the Japanese artists are very partial. These are the principal alloys, but there are several varieties of them, as well as combinations of shaku-dō and shibu-ichi in various proportions, as, for instance, in the case of kiu-shibu-ichi, the composition of which would correspond to one part of shaku dō rich in gold, and two parts of shibu-ichi rich in silver. Interesting effects are produced by pouring two alloys of different tints together just at the solidifying point of the less fusible of the two, so that the alloys unite but do not blend, and a mottled surface is the result. These alloys are introduced into almost every good piece of metal-work.

Shaku-dō.

I.	
Copper	94·50
Silver	1·55
Gold	3·73
Lead	·11
Iron and Arsenic	traces
	99·89

II.	
Copper	95·77
Silver	0·08
Gold	4·16
	100·01

Shibu-ichi.

III.	
Copper	67·31
Silver	32·07
Gold	traces
Iron	·52
	99·90

IV.	
Copper	51·10
Silver	48·93
Gold	·12
	100·15

Now as to the action of pickling solutions. Many of you will be familiar with the mysteries of the treatment of brass by "dipping" and "dead dipping," so as to produce certain definite surfaces, but the Japanese art metal-workers are far ahead of their European brothers in the use of such solutions.

The South Kensington Museum contains a very valuable series of fifty-seven oblong plates, some plain and others richly ornamented, which were specially prepared as samples of the various metals and alloys used by the Japanese. The Geological Museum in Jermyn Street has a smaller, but very instructive, series, of twenty-four plates presented by an eminent metallurgist, the late M. Hochstätter-Godfrey. From descriptions accompanying the latter, and from information I have gathered from certain Japanese artificers now in London, it would appear that there are three solutions generally in use. They are made up respectively in the following proportions, and are used boiling.

	I.	II.	III.
Verdigris	438 grains	87 grains	220 grains
Sulphate of copper	292 ,,	437 ,,	540 ,,
Nitre	—	87 ,,	—
Common salt	—	146 ,,	—
Sulphur	—	233 ,,	—
Water	1 gallon	—	1 gallon
Vinegar	—	1 gallon	5 fluid drachms

That most widely employed is No. I. When boiled in No. III. solution, pure copper will turn a brownish red; and shaku-do, which, you will remember, contains a little gold, becomes purple; and now you will be able to appreciate the effect of small quantities of metallic impurity as affecting the colour resulting from the action of the pickle. Copper containing a small quantity of antimony gives a shade very different from that resulting from the pickling of pure copper. But the copper produced in Japan is the result of smelting complex ores, and the methods of purification are not so perfectly understood as in the West. The result is that the so-called "antimony" of the Japanese art metal-workers, which is present in the variety of copper called "kuromi," is really a complex mixture containing tin, cobalt, and many other metals, so that a metal-worker has an infinite series of materials at command with which to secure any particular shade; and these are used with much judgment, although the scientific reasons for the adoption of any particular sample may be hidden from him. It is strictly accurate to say that each particular shade of colour is the result of minute quantities of metallic impurity, and these specimens and diagrams will, I trust, make this clear, and will prove that the Japanese arrange true pictures in coloured metals and alloys.

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[1] A list of books and papers dealing with the colours of metals and alloys, and with the production of coloured patina, is given by Prof. Ledebur in his work "Die Metallverarbeitung," p. 285, 1882, published in Bolley's "Technologie."

[2] Analyses Nos. I. and III. are by Mr. Gowland, of the Imperial Japanese Mint at Osaka; Nos. II. and IV. by Prof. Kalischer, *Dingl. Polyt. Journ.*, ccxv. 93.

[This portion of the subject was illustrated with much care by coloured diagrams representing specimens of Japanese art metal-work, by photographs projected on the screen, as well as by the reflected images of small ornaments made of the alloys which had been specially referred to. There was also a trophy of leaves of copper of varying degrees of purity coloured brilliantly by one or other of the "pickles" above described.]

There is one other art material to the production of which I hope art workmen in Birmingham will soon direct their attention, as its applications are endless. It is called in Japanese "mokume," which signifies "wood-grain." It is now very rare even in Japan, but formerly the best specimens appear to have been made in Nagoya by retainers of the Daimio of Owari. I have only seen six examples, and only possess a single specimen of native work, and have therefore had to prepare a few illustrations for you in soldered layers of gold, silver, shibu-ichi, shaku-dō, and kuromi.

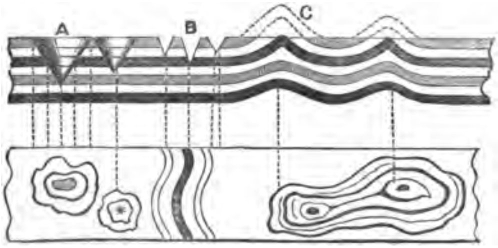

Fig. 3.

This diagram (Fig. 3) shows the method of manufacture. Take thin sheets of almost any of the alloys I have mentioned, and solder[1] them together layer upon layer, care being taken that the metals which will present diversity of colour come together. Then drill conical holes of varying depth, A, in the mass, or devices in trench-like cuts of V section, B, and hammer the mass until the holes disappear; the holes will thus be replaced by banded circles and the trenches by banded lines. A Japanese artificer taught me to produce similar effects by taking the soldered layers of the alloy, and by the aid of blunted tools making depressions on the back of the mass so as to produce prominences on the front, C. These prominences are filed down until the sheet is again flat; the banded alloys will then appear on the surface in complicated sections, and a very remarkable effect is produced, especially when the colours of the alloys are developed by suitable "pickles." In this way any device may be produced. In principle the method is the same as that which produces the *damascening* of a sword-blade or gun-barrel, and depends on the fact that under certain conditions metals behave like viscous solids, and as truly "*flow*" as pitch or honey does, only in the case of mokume the art workman has a wide range of tinted metals at command.

Throughout Japanese art metal-work, in which I hope you will take increasing interest, there is the one principle of extreme simplicity and absolute fidelity to nature. The brilliant metals, gold and silver, are used most sparingly, only for enrichment, and to heighten the general effect; these precious metals are never allowed to assert themselves unduly, and are only employed where their presence will serve some definite end in relation to the design as a whole. A Japanese proverb asserts that "He who works in gold puts his brains into the melting-pot," meaning, I suppose, that this metal, so precious from an artistic point of view, demands for its successful application the utmost effort of the workman, and suggesting that gold should not be employed in massive forms such as would result from melting and casting, but should be daintily handled, beaten on to the work, or embedded with the hammer.

Bear in mind that in Birmingham, when a really fine work is produced in silver, the surface is often made gray by chemical means, "oxidised," as it is termed, and this subordination of the brilliancy of silver to artistic effect, is well understood by the celebrated American firm, Messrs. Tiffany, of New York, who are doing so much to catch the spirit of Japanese art metal-work. All I ask you to do is to carry this still further—to cover base metals with these glowing coloured oxides, and thus to add to the permanence of art work, by producing surfaces which will resist the unfavourable atmospheric influences of our cities.

Hitherto we have considered the union of metals by fusion, but fire is not the only agent which can be employed for this purpose. Two or more metals may be deposited side by side by the aid of the electric battery. Birmingham was, as you well know, the early home of electro-metallurgy, an industry to the development of which the great firm of Elkington has so materially contributed. I have no statistics as to the amount of precious metals annually employed for electro-deposition in Birmingham, but it is known that a single works in Paris, belonging to M. Christofle, deposits annually six tons of silver, and it has been estimated that the layer of silver of the thickness actually deposited on various articles would, if spread out continuously, cover an area of 140 acres.[1] I will not, however, dwell upon the deposition of gold and silver in their normal colours. I would remind you that copper and zinc may be deposited by electrolysis so as to form brass, and that all the beautiful bronzes and alloys of the Japanese can be obtained by galvanic agency; and further, by suitable admixtures of gold, silver, and copper, red-gold, rose-coloured gold, or green gold may be deposited, so that the electro-metallurgist has at his command the varied palette of the decorative artist.

[The images of beautiful deposits of coloured gold, specially prepared by Messrs. Elkington, were then projected on the screen.]

I ought to allude to what has been called the moral aspect of colour, and although I cannot follow Goethe[2] in his attributes of colour, which seem to me to be fantastic and over-strained, I quite recognise the poetic sympathy of Shakespeare in making Bassanio select the casket of lead, which contained the warrant for his earthly happiness, because "its paleness moved him more than eloquence." I ask you to remember Ruskin's words, that "all men completely organised and justly tempered enjoy colour; it is meant for the perpetual comfort and delight of the human heart; it is richly bestowed on the highest works of creation, and the eminent sign and seal of perfection in them being associated with life in the

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[1] The following solder was found to answer well:—

Silver	55·5
Zinc...	26·0
Copper	18·5
	100·0

[1] H. Bouilhet, *Ann. de Chim. et de Phys.* t. xxiv. p. 549, 1881.

[2] Farbenlehre.

human body, with light in the sky, with purity and hardness in the earth; death, night, and pollution of all kinds being colourless."

I must briefly turn to the concluding part of our subject. It has long been known that thin films of certain metals and certain metallic oxides act on light in the same way as thin films of other translucent substances. I have here such thin films of oxide of lead, which, many years ago, Nobili, Becquerel, and Gassiot taught us to deposit, and such films have since been used in decorative metal-work.

[Beautiful examples of such films were projected on the screen.]

I wish I had time to point to the great interest and importance of films of coloured oxide of iron in the tempering of steel, for it is well known that, apart from the scientific interest of the subject, the shades from straw-colour to blue which pass over the surface of hardened steel when it is heated in air, afford precious indications as to the degree of temper the metal has attained, and in no industry is this better shown than in the manufacture of steel pens. I must pass this over, and turn to one other instance of the formation of coloured films on metals. Here is an ordinary plumber's ladle filled with lead, which will soon be molten when it is placed over this flame. The air will play freely on the surface of the melted lead, and, as a certain temperature is reached, very beautiful films will pass over the surface of the metal. If the lead contains very minute quantities of cadmium or of antimony, the effect will be greatly heightened. If the light from the electric lamp be allowed to fall on the surface of the bath of lead, it will be easy to throw the image of the metallic surface on the screen, and you will see how beautiful the films are and how rapidly they succeed each other when the metal is skimmed. What, then, is the special significance of the experiment from our point of view? It represents in a singularly refined way the one experiment which stands out prominently in the whole history of chemistry; for the formation of a coloured scum on lead when heated in air has been appealed to, more thany any other fact, in support of particular sets of views from the time of Geber in the seventh century to that of Lavoisier in the eighteenth. It was the increase in weight of the lead when heated in air that so profoundly astonished the early chemists; and, finally, the formation of a coloured oxide by heating lead in air was the important step which led on your great townsman, Priestley,[1] to the discovery of oxygen; and, as the fact of his residence among you will never be forgotten, Birmingham may claim to have been connected, through him, with one of the most splendid contribution ever offered to Chemical Science.

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[1] He pointed out that the experiment with minium confirmed his view that he mercury calcined in air derived oxygen *from* the air.

NOTES

PROF. RÜCKER, F.R.S., has been appointed by the Lord President of the Council to the Professorship of Physics in the Normal School of Science and Royal School of Mines, rendered vacant by the death of Prof. Guthrie, F.R.S.

AT the Royal Society on Thursday last (November 25) a paper was read by Sir Richard Owen, containing some further evidence on the structure of the very remarkable extinct marsupial, *Thylacoleo carnifex*. The author re-affirmed his previous statements that it was a carnivorous beast of the size of a lion, the probable prey of which had been the larger herbivorous marsupials, also now extinct. Prof. Flower, after reviewing the additional evidence that had been adduced, repeated his conviction expressed eighteen years ago in a paper read before the Geological Society, that the dentition of *Thylacoleo* found no parallel in any existing predaceous carnivore, but was formed on a totally different type, and that there was therefore no justification for assigning to it habits for which it did not seem particularly well adapted. The essential conditions in a dentition which would enable an animal to seize and overcome large and struggling prey, as seen in both lions, tigers, wolves, and the existing carnivorous marsupials, are large canines set well apart, with incisors so small as not to interfere with their piercing action; whereas in *Thylacoleo* the canines are rudimentary, and the central incisors greatly developed. The alternative, sometimes suggested, that the animal was herbivorous, was equally improbable. In fact, it would not be safe to do more than speculate on the habits or food of an animal the dentition of which was so highly specialised, and without any analogy in the existing state of things. Prof. Huxley said that he agreed with the conclusions of the last speaker.

A COURSE of six lectures, adapted to a juvenile auditory, on "The Chemistry of Light and Photography" (with experimental illustrations), will be given at the Royal Institution by Prof. Dewar, M.A., F.R.S., on the following days, at three o'clock:—Tuesday, December 28, 1886; Thursday, December 30; Saturday, January 1, 1887; Tuesday, January 4; Thursday, January 6; Saturday, January 8.

THE Royal Society have just received from Egypt a consignment of specimens of the different strata of soil in the Delta. The borings have been carried out to a depth of nearly 200 feet, and the solid bottom has not yet been reached. The Royal Engineers in Egypt have been intrusted with the work. The specimens, which are chiefly of sand and clay strata, are deemed of great importance, and the Society has granted money for the continuance of the work, which will be carried out by the detachment of Engineers as hitherto.

THE Secretary of State for War has given permission for Sir Frederick Abel, C.B., the Chemist of the War Department, to accept the post of organising secretary to the Imperial Institute, provided that the duties do not interfere with those of his appointment under the War Office; and Sir Frederick Abel has been desired by the Prince of Wales, President of the Imperial Institute, to enter upon his work as soon as possible. The new secretary has just completed his work in connection with the electric lighting of the Indian and Colonial Exhibition, and is also retiring from his duties in connection with the Society of Arts.

ON November 17, at 7h. 18m. p.m., a fine fireball was seen at Stonyhurst College, Blackburn. It appeared to be several times as bright as Venus. In colour it was violet, and of a distinct pear shape. The part of its path observed, as far as could be judged from the stars seen through detached clouds, was from near ι Ceti to the small stars above Fomalhaut, about 88 Aquarii. Its path was slightly curved. So brightly did it shine that attention was first called to it by the illumination of the sky, although seen from a room in which the gas was lighted.

THE *Morning Star* of Jaffna, in Ceylon, reports the death of the taxidermist of the Victoria Museum in that town from the bite of a cobra, under very curious circumstances. While feeding a cobra, which he had supposed was harmless from previous extraction of the poison-bag, it suddenly bit his hand. For a few minutes he took no notice, thinking the bite harmless, but pain and nausea soon began. Carbolic acid was applied, ligatures were bound round the arm, an incision was made at the bite, and the blood of the arm was wholly removed. Various antidotes were used, but the unfortunate man lost the power of speech, and soon after every muscle seemed to have become paralysed, and breathing entirely ceased.

Artificial respiration was therefore resorted to, and this operation was unceasingly continued for nine hours, when at last the patient made an attempt to breathe, and soon regained consciousness enough to make his wants known. He steadily improved until the Friday, the accident having taken place on a Wednesday, and then astonished those around him by stating that during the severe operation of Wednesday night he was conscious of all that was taking place, but was unable to make his feelings known, not having power over a single muscle. It would seem that the poison paralysed the nerves of motion, but not those of feeling, for he could see, and hear, and feel, although the physicians, even by touching the eyeball, could get no response either of feeling or consciousness. His partial recovery was, however, followed by a high fever and inflammation of the lungs, and he died, perfectly conscious, on the following Sunday.

THE New Zealand Government are about to collect salmon ova in Scotland and transfer them to that colony for incubation. It will be remembered that the Royal Commissioner for New Zealand has previously carried out similar work successfully, and it has been found that the *S. salar* thrives well in the waters of that possession. Last year a large number of salmon ova were collected from Scotland, and hatched out and reared in New Zealand.

COMMENCING on January 1, 1887, a journal is to be published by the National Fish-Culture Association, comprising not only information regarding its transactions from time to time, but also articles relative to the subjects of fish-culture, fish, and fisheries. A record will also be given of what takes place in connection with these subjects throughout the whole of the United Kingdom, the colonies, and abroad.

A STRONG shock of earthquake, lasting several seconds, was felt at Smyrna and in the adjacent districts early on the morning of November 27, and news has been received of Tchesme and Chios having been similarly visited. A strong shock was felt at Tashkend on the morning of November 29, causing damage to many houses in the Russian quarter. Two shocks were felt on Sunday at Somerville and at Charleston. A slight shock was felt in Cairo at half-past four o'clock on the afternoon of the 17th. The vibration lasted several seconds.

DURING the past summer, Dr. Fr. Svenonius, the well-known Swedish geologist, has been prosecuting geological, ethnographical, and glacial studies in Swedish Lapland.

ON the evening of November 4 a splendid display of the aurora borealis was seen at Throndhjem, in Norway. Not only the northern, but also the eastern and part of the southern, sky were covered with aurora. The radiation was particularly brilliant from south-west to north-east, forming a wreath in all the colours of the rainbow. During October, several splendid displays of aurora occurred, but none as brilliant as this one.

ON the evening of October 30 a brilliant meteor was seen from the Falsterbo lightship, on the south-west coast of Sweden. It went in a direction south-south-west to north-north-east, exploding, as it seemed, from time to time, and displaying the most brilliant yellow, red, and green light. At times the sky was illuminated as in full moonlight. About a couple of minutes after the last explosion, reports as of guns were heard. At about 2 a.m. of November 5 another splendid meteor was seen at Hamar, in Norway. It went in a southerly direction across Lake Mjösen, and disappeared from view, leaving a long, broad, variegated trail behind.

PROF. COLLETT, the well-known Norwegian zoologist, announces that the beaver is now extinct in Northern Norway, but estimates that about 100 are still in existence in the south, chiefly in the province of Nedenaes.

A KITCHEN-MIDDEN has just been discovered at Ginnerup, in Denmark, at the foot of a cliff near a dried-up sound. It is about a yard in depth and of considerable extent, and contains quantities of shells of oysters, mussels, &c.

THE last numbers of the *Journal* of the China Branch of the Royal Asiatic Society (vol. xxi. Nos. 1 and 2) contain a "Symposium" on the question whether Western knowledge, and especially, of course, Western science, should be conveyed to the Chinese through the medium of their own or of a Western language. Fourteen of the leading European scholars in China took part in the discussion. Their views will not bear classification under the heads affirmative or negative, as some hold a middle place, exhibiting a leaning in one direction or another. The general tendency, however, is in favour of exciting the curiosity and interest of intelligent Chinese in the matter of Western knowledge by popular exposition in the native tongue, while reserving a more adequate representation for a time when a sufficient number of Chinese shall have acquired foreign languages to constitute a learned class in our sense of the expression. A further and final stage will be reached when the members of this class, themselves impregnated with foreign knowledge, shall convey it to their fellow-countrymen in their own tongue, gradually modified to suit the exigencies of doctrines now absolutely foreign to the genius of the Chinese language and beyond its capabilities.

IN the course of the discussion, some interesting facts with regard to the translation of scientific terminology into Chinese were mentioned. Dr. Martin, of Pekin, referred to Ricci's old translation of Euclid, which he entitled "The Fundamental Principle of the Science of Quantity." Oxygen, hydrogen, and nitrogen are translated so as to express their characteristics of supporting life, of lightness, and of derivation from nitre. On the other hand, Dr. Macgowan mentions that a translator's difficulties in dealing with natural history terms are really enormous. He undertook the translation of Dana's "Mineralogy" and Lyell's "Elements of Geology" into Chinese for the Government, and a scientific native scholar was detailed to assist him. When they came to the plants that have the names of foreign botanists, most of them polysyllabic, they were appalled, and as they could only be rendered phonetically, the native scholar decided against translating any portion of the plant's name, transferring it bodily, according to sound, into Chinese. Similarly, the complex nomenclature of organic chemistry presents a formidable difficulty. A Chinese clergyman, who took part in the discussion, delivered a particularly interesting address, urging that the phonetic method should, as a rule, be employed, on the ground that the characters used in the translation of scientific terms have traditional meanings to the Chinese mind, and thus great confusion is created. The "term"-controversy which has agitated theologians in China for the past half-century, and has divided them into two hostile camps, appears likely to revive in the domain of science, the question lying between translation or phonetic reproduction.

FROM a study of thirty-two years' observations of thunderstorms in the Vienna region, Dr. Hann finds that there is a double maximum of frequency. The greatest number occur in the first half of June, the second smaller maximum is in the end of July; between these is a secondary minimum. (Thunderstorms hardly ever occur in winter.) This agrees with observations in Munich. In Brussels most thunderstorms occur in the second halves of June and July. The daily period in Vienna shows a chief maximum about 3.20 p.m., and a secondary one at 1.2 a.m. The spring and summer storms come mostly from the east or south-east, and seem to belong to Mediterranean depressions, coming up from the Adriatic, as those of late summer seem to be on the south or south-east border of Atlantic depressions.

Bethnal Green Free Library has been doing a large amount of good work in the thickly-populated district in which it is situated, not only by giving facilities for reading books, but by science lectures and science "talks." It is much in want of funds for the extension of operations, and we commend it to the consideration of our readers. The librarian is G. F. Hilcken.

The additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*) from India, presented by Miss G. M. Fisher; a —— Hedgehog (*Erinaceus* ——) from Madras, presented by Mr. H. R. P. Carter; two Mute Swans (*Cygnus olor*), European, a Common Peafowl (*Pavo cristatus*) from India, presented by Lady Siemens; a Red and Yellow Macaw (*Ara chloroptera*) from South America, presented by Mr. Arthur Daunt; a Grey Parrot (*Psittacus erithacus*) from West Africa, presented by Mrs. Greenwood; five Great Eagle Owls (*Bubo maximus*), European, presented by Mr. Philip Crowley, F.Z.S.; a Common Guillemot *Lomvia troile*), British Islands, presented by Mr. J. H. Gurney, F.Z.S.; two Gambel's Partridges (*Callipepla gambelli*) from California, presented by Mr. W. A Conklin, C.M.Z.S.; a Malabar Green Bulbul (*Phyllornis aurifrons*) from India, received in exchange; five Great Titmice (*Parus major*), four Blue Titmice (*Parus cæruleus*), two Bullfinches (*Pyrrhula europæa*), European, purchased.

OUR ASTRONOMICAL COLUMN

The Argentine General Catalogue of Stars.—This Catalogue, containing the mean positions of 32,448 southern stars determined at the National Observatory of Cordoba, has recently been published by Dr. Gould. The observations from which the Catalogue positions are deduced were made with the meridian-circle of the Cordoba Observatory during the years 1872–80. During these years the zone-observations were the chief object of attention, and the present Catalogue contains the places of those stars whose positions were more elaborately determined during the progress of that great work, and constitute an addition to our knowledge of southern stellar positions of perhaps not less importance than the Cordoba Zone-Catalogue. The General Catalogue gives the positions, for the epoch 1875·0, of most of the southern stars brighter than magnitude 8½, the deficiencies in this respect being chiefly found north of the parallel of 23°, at which the zones begin. These omissions will be of comparatively small importance, inasmuch as the new *Durchmusterung* of Prof. Schönfeld comprises all the southern stars within this region, while accurate determinations of the brighter ones will have been made in the re-observation of Lalande's stars now nearly completed at the Paris Observatory.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 DECEMBER 5–11

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 5

Sun rises, 7h. 51m.; souths, 11h. 50m. 51·4s.; sets, 15h. 50m.; decl. on meridian, 22° 25′ S.: Sidereal Time at Sunset, 20h. 47m.

Moon (two days after First Quarter) rises, 13h. 30m.; souths, 19h. 35m.; sets, 1h. 51m.*; decl. on meridian, 0° 19′ N.

Planet		Rises		Souths		Sets		Decl. on meridian
		h. m.		h. m.		h. m.		° ′
Mercury	...	7 16	...	11 32	...	15 48	...	19 53 S.
Venus ...	...	7 54	...	11 53	...	15 52	...	22 27 S.
Mars ...	...	10 19	...	14 10	...	18 1	...	23 45 S.
Jupiter ...	...	3 34	...	8 49	...	14 4	...	9 41 S.
Saturn ...	...	18 35*	...	2 38	...	10 41	...	21 29 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

Dec.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image	
			h. m.	h. m.	°	°
5	14 Ceti ...	6½	16 52	near approach	358	—
10	48 Tauri ...	6	5 52	6 38	105	339
10	B.A.C. 1526	6	22 29	23 44	78	292

Saturn, December 5.—Outer major axis of outer ring = 45″·4; outer minor axis of outer ring = 17″·7; southern surface visible.

Variable Stars

Star	R.A.	Decl.				
	h. m.	° ′			h. m.	
U Cephei	0 52·2	81 16 N.	Dec.	8,	1 46	*m*
Algol	3 0·8	40 31 N.	,,	6,	23 15	*m*
			,,	9,	20 5	*m*
ζ Geminorum ...	6 57·4	20 44 N.	,,	9,	5 0	*m*
V Geminorum ...	7 16·8	13 19 N.	,,	7,		*M*
U Coronæ	15 13·6	32 4 N.	,,	7,	0 32	*m*
β Lyræ...	18 45·9	33 14 N.	,,	11,	19 0	*M*
S Vulpeculæ ...	19 43·7	27 0 N.	,,	6,		*m*
T Aquarii	20 43·9	5 34 S.	,,	5,		*M*
δ Cephei	22 24·9	57 50 N.	,,	7,	0 0	*M*
			,,	10,	19 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers

The principal shower of the week is that of the *Geminids*; R A. 105°, Decl. 32° N., but moonlight will interfere with its observation at the time of its maximum, December 10–11.

Stars with Remarkable Spectra

Star	R.A. 1886·0	Decl. 1886·0	Type of spectrum
	h. m. s.	° ′	
20 Leporis	5 6 3	11 59·4 S.	III.
119 Tauri	5 25 32	18 30·5 N.	III.
64*a* Schjellerup ...	5 38 52	20 38·8 N.	IV.
α Orionis	5 48 59	7 23·1 N.	III.
π Aurigæ	5 51 27	45 55·5 N.	III.

THE ROYAL SOCIETY[1]

For many years it has been my duty as senior secretary to read at each anniversary the death-roll of the year. The names this year are perhaps slightly fewer than usual, but many recall to us faces once familiar that we shall never see here again. Earliest among them comes Sir Frederick Evans, whose death took place only very shortly after our last anniversary. In the course of the preceding summer he crossed the Atlantic to take part in that International Conference which assembled at Washington, to deliberate among other things on the choice of a common prime meridian for all civilised nations. On his return he was looking ill, and the illness increased until it carried him away. Yet even through his illness he kept on working at science, at a task he had undertaken, and which was almost completed when he died. To this I shall have occasion to refer again. In Mr. Busk we have lost one who has long been among us, and who took an active part in the scientific business of the Society. He repeatedly served on our Council, and both then and subsequently gave us the benefit of his extensive knowledge and sound judgment in the important but laborious task of advising the Committee of Papers as to the proper mode of dealing with papers which they referred to him. In Lord Cardwell we have lost a statesman whose political duties did not prevent him from coming among us and serving on our Council. The public services and singular honesty and straightforwardness of Mr. Forster are appreciated by the nation at large. Quite recently, at no advanced age, we have lost Prof. Guthrie, the occupant of a chair which a great many years ago I held for a time; a man whose genial character drew around him a close circle of friends. Still more recently we have lost the Earl of Enniskillen, whose fine palæontological collections are well known to geologists. Only the other day one passed away whom we seldom missed at our anniversary meeting, and who was frequently with us on other occasions: I allude to General Boileau, whose philanthropic labours will not soon be forgotten, and may, I trust, be recognised in a much needed form.

The Fellows will have noticed with satisfaction a very considerable excess of income over expenditure in the balance sheet for the year. At first sight it might be supposed that as the *Transactions* come out at irregular intervals there might have been fewer parts published than usual; but it will be found on examination that the past year has borne its proper share of printing expenses. The excess is really due to a substantial improvement in the Society's property, under the careful and judicious management of our Treasurer.

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[1] Anniversary Address by Prof. G. G. Stokes, President, on Tuesday, November 30, 1886.

Last year our President informed the Fellows of the munificent offer made to the Society by Sir William Armstrong to give to the Scientific Relief Fund the sum of 6500*l.*, provided an equal sum were raised by subscription from the Fellows, and, if need be, other friends of science who might not belong to the Society. As the Fellows are aware, a circular was sent round by the Treasurer mentioning Sir William Armstrong's generous offer, and inviting subscriptions; and the Treasurer has also written privately to a number of persons, Fellows and others. The sum subscribed or promised in response to this invitation amounts to about 4200*l.*; and though the sum thus raised does not amount to what Sir William promised to duplicate if it could be raised, he has most generously not only waived the non-fulfilment of the condition subject to which the former offer was made, but has still further augmented it; and he now promises not only to duplicate the sum raised in answer to the Treasurer's appeal, but to give the further sum of 3600*l.*, thus raising the capital from the present sum of about 8000*l.* to 20,000*l.* He will be ready to pay the whole sum of 7800*l.* as soon as the subscriptions promised to the Treasurer have been collected. The only condition attached to this princely gift is, that, besides meeting the ordinary objects for which the Fund was instituted, the Council should feel themselves at liberty to apply a portion of the income to defraying the annual subscriptions of Fellows in special cases where such a course might seem desirable.

The path of the total eclipse of September 9 of last year, in any place where it fell on land, was so remote from this country that no expedition went out to observe it. It was visible in New Zealand, and in anticipation of it our Eclipse Committee sent out a memorandum to the colony indicating the points of special interest to look out for. We have received accounts, drawings, and photographs of the eclipse from Dr. Hector and others. One of the most remarkable features of this particular eclipse was the appearance of two white and unusually bright prominences which attracted general notice, and were compared to electric lamps, and which, situated on opposite sides of the sun, were just over the places of two large spots, one close to the limb, and on the point of disappearing, the other not seen before the eclipse but visible next day, having in the meantime come round the limb.

The present year afforded another of those rather rare occasions, always of brief duration, which are afforded for the study of solar physics by a total eclipse of the sun. Calculations made long beforehand showed that a total eclipse was to be expected on August 29. The path of the total phase on the earth's surface is always narrow, say 100 miles or so across, and on this occasion it swept obliquely across the Atlantic Ocean, cutting the Western Coast of Africa about Benguela, and sweeping across some of the West India Islands to a part of the mainland of South America, where it ended.

Though there was a long belt of ocean over which the totality would be visible, and where the imposing spectacle of a total eclipse might be witnessed, this was not available for regular scientific observations, which require land on which to fix the instruments. On the mainland of America the total phase would come on so shortly after sunrise that the sun would be too low for good observations, and therefore the Island of Grenada, which lay within the belt of totality, was much to be preferred.

Of the two available stations, one lay in the British dominions, and was pretty easy of access from England, and accordingly it seemed to be the duty of our country to take a foremost place in the observations, the results of which would be available to the whole scientific world. It was contemplated first to send expeditions to both places—to Benguela as well as Grenada. The cost of this would, however, exceed what could be spared from the Government Grant without unduly curtailing what was available for other objects. Accordingly application was made to the Lords of the Treasury for a grant of 1000*l.* towards the cost of the expeditions. Inquiries were also made as to the probable climate at the two places; and here I have to express our thanks to the Governor-General of the Windward Islands, who put us in communication with Dr. Wells of Grenada, from whom we obtained valuable information regarding the climate of that island, and to the Consul-General for Portugal, who obtained information for us from the Polytechnic Institution at Lisbon as to the amount of sunshine about the end of August at Loanda, which may be taken pretty well as representing Benguela.

The information we obtained from various sources as to Benguela was rather conflicting, but there seemed a pretty general agreement that even if the sun should be shining at the time of the eclipse the sky was likely to be hazy. This would much interfere with good observations, especially as regards the corona; and as the expense of the expedition to Benguela would be considerable, and the success very doubtful, we thought it better to give up that part of the project and confine ourselves to Grenada. Being anxious to trench as little as possible on the national expenditure, and finding that a little more could be taken from the Government Grant than we had expected, we wrote to the Treasury reducing our application to 500*l.*, which, with assistance from the Admiralty in the shape of the use of a ship-of-war on the West India station, and supplemented by some money from the Government Grant and from our own Donation Fund, might enable us to meet the expenditure.

The result was that a sum not exceeding 500*l.*, to supplement what could be spared from the Government Grant, was granted, and the expedition, as the Fellows are aware, has sailed and returned. It was fairly successful, the observations having been prevented by clouds at only one of the stations occupied.

There has not yet been time to discuss the observations in full, but two points already appear to have come out pretty clearly. One is that the brightness of the corona, which on this occasion was actually measured, was much less than had been expected, and less apparently than it had been on former occasions. This seems to show that the brightness is liable to great changes in comparing different years, as we know is the case with the form. The other point touches on the question of the possibility of photographing the corona independently of an eclipse. If the photographic brightness of the corona be not overpowered by that of the atmospheric glare immediately around the sun when there is no eclipse, then when the sun is partially eclipsed we might expect to be able to trace the outline of the limb of the moon for some way outside the sun, since the moon would be projected on the background of the corona. The experiment was tried both by Capt. Darwin at Grenada, and by Dr. Gill at the Cape, but in neither case was the limb traceable outside the sun. This throws doubt on, but does not disprove, the validity of the method; for Dr. Huggins himself has never obtained photographic appearances apparently referable to the corona since the Krakatão eruption. It may be that the finely suspended particles, whether connected with the Krakatão eruption or not, which produced those gorgeous sunsets that were so remarkable, have not yet wholly subsided, and cause a considerably increased atmospheric glare. It may be that the corona has actually been much less bright than usual for the last few years.

The present year has been signalised by that remarkable volcanic explosion in New Zealand, of which we have read accounts in the newspapers. We have received from Dr. Hector a series of photographs of the district, taken at no great length of time after the explosion.

The Krakatão Committee, which was appointed at the suggestion of our late President to collect information relative to the great eruption, have now I may say completed their work. The Royal Meteorological Society had appointed a Committee to get together information respecting the remarkable atmospheric phenomena witnessed after the eruption. It was thought desirable that the two Committees should work in concert, and accordingly our Committee was enlarged by the addition of two members of the Royal Meteorological Society, even though they did not happen to be members of the Royal Society, who undertook that share of the work. The information collected under this head is naturally voluminous, since it requires no special training to describe the atmospheric appearances. Our late Fellow, Sir Frederick Evans, undertook the sea-disturbance, and continued to work at it even in an advanced stage of the disease which carried him off. Another fortnight, it was estimated, would have enabled him to complete it. His account was found to have been written in pencil on separate sheets of note-paper, but his successor in the office of Hydrographer, Captain Wharton, our Fellow, was so good as to take up the work; and partly by the use of materials left by Sir F. Evans, partly by his own independent labour, he has now completed it. The report on air-disturbance was undertaken by General Strachey, and is ready. Prof. Judd undertook geology; the materials are ready, and though the actual report is not yet written, the writing would take but very little time. Mr. Scott undertook to collect information as to floating pumice; but as it has been found that the Krakatão pumice does not possess distinctive features whereby it could be recognised, and therefore the origin of the pumice that ships have encountered at a distance from Krakatão remains unknown, little trustworthy information could be obtained under this head, and the report has been handed over to Prof. Judd to embody with the geology. The heaviest part of the report, that relating to sunsets and atmospheric phenomena, has been prepared by the Hon. Rollo Russell and Prof. Archibald, the two Fellows of the Royal Meteorological Society who have been mentioned as having been added to the Committee, and is ready, with the exception of a little revision, and it remains only to prepare an introduction, index, &c. The whole report may therefore be regarded as all but complete in manuscript, and it will be for the new Council to deal with it.

The Circumpolar Committee have now brought their labours to a close, the report on the observations taken by Capt. Dawson at Fort Rae being printed and published. The reports of the expeditions undertaken by Austria, the United States of America, Germany, and Holland are, I understand, complete, and those by France and Russia are in a forward state. Before the accounts of the observations taken at different stations by the observers of different nations shall have been for some time before the public, it would be premature to expect general conclusions to be deduced from this great undertaking.

Very satisfactory progress has been made during the past year with the publication of the Report of the *Challenger* Expedition. The volumes already published and in the Society's Library now amount to sixteen on Zoology, and three introductory on other subjects. Others are in a very forward state, and it is expected that the whole will be published very nearly within the time mentioned by the Committee, probably at the end of the next financial year.

As mentioned in the Presidential Address last year, advantage has been taken of the British occupation of Egypt to make some explorations by way of boring in the Delta of the Nile, to the results of which geologists attach great importance. The War Department has allowed some of the staff of the Royal Engineers, when their services were not otherwise required, to take part in the operations, and has lent the boring apparatus, and the Royal Society voted the sum of 350*l.* out of its own Donation Fund to defray the cost of labour and other incidental expenses. It was contemplated originally to make a chain of borings, but the depth to which it has been found necessary to proceed in order to get through the ordinary deposit has turned out to be so great that it was thought better, instead of attempting many, to try and get if possible down to rock, or to something else which might afford evidence that what could be referred to alluvium from the Nile or drifted sand had really been got through. A deep boring has accordingly been made at Zagazig, under the direction of Capt. Dickenson, R.E. This has now been carried to a depth of 190 feet 6 inches below the surface, or 164 feet 5 inches below the mean sea-level at Alexandria, and yet nothing has been reached but sand and clay with small pebbles. Prof. Judd is now engaged in the examination of the matter brought up. A derangement of the boring apparatus prevented for the present further progress, and the use of a narrower pipe than any at hand would be required for carrying the boring deeper. The Committee considered that it would be more important to extend this boring, so as if possible to get down to rock, or else to Miocene fossils, than to make a fresh boring in a different place, and arrangements are being made accordingly. The inquiry was deemed a proper one to be assisted out of the Government Grant, and the sum of 200*l.* has been voted from this source to supplement the Royal Society's grant already mentioned.

The ordinary meetings of the Society are well known, and are frequently attended by strangers by permission of the Fellows present; and the papers brought before us are known to the world through our publications. But a great deal of scientific work is done of which the outside public knows nothing. There have been thirteen meetings of the Council during the year, and the attendance at our council meetings is remarkably good. There have been more than seventy meetings of committees and sub-committees.

There is further another task on which a great deal of gratuitous and conscientious labour of the highest kind is bestowed. I allude to the examination of papers with a view to advising the Committee of Papers as to their publication. The past year has shown no flagging in scientific activity in relation to papers brought before us.

The preparation of the manuscript for another decade, 1874 to 1883, of the Royal Society's catalogue of scientific papers, is now almost complete. This great work has been extremely useful to men of science in enabling them at once to find where a memoir on a particular subject, written by an author whose name they know, as is usually the case, is to be found. To some extent it enables them also to find what has been written on a particular subject, for there are usually one or two authors, whose names they know, who have made it a special study, and on consulting their papers references are frequently found to the writings of others who have written on the same subject. Nevertheless, it must be confessed that the value of the catalogue would be greatly increased if it could be accompanied by a key, of the nature of an *index rerum*. It was originally contemplated that this should be added, but the magnitude of the undertaking has hitherto prevented the Committee from attempting it. To be well done it would require the long-continued labour of a scientific staff representing different branches of science, and they could not be expected to engage in so heavy a work without adequate remuneration.

A great deal of work has been done during the past year in relation to the library. More than 5000 volumes have been removed to other rooms to make space for the more important works constantly accruing. A list of duplicates and deficiencies has been printed and circulated among corresponding societies. A shelf catalogue is in progress, and is about a third of the way towards completion. Some work has also been done upon a catalogue of miscellaneous literature.

The electric lighting of the Society's apartments, which is now complete, seems to have given general satisfaction.

On August 31 of this year, our distinguished Foreign Member, M. Chevreul, attained his hundredth year. Rarely indeed is it given to any one to see right through a century, more rarely still to retain his powers to such an age, yet both, I am happy to say, have been granted to M. Chevreul. In anticipation of this event, preparations were made for its due celebration. I received an invitation for our Fellows to assist at the celebration; but unfortunately it was at a time of year when most of us were scattered, and moreover time did not permit of making it generally known. I am afraid we had no representative at the actual ceremonial, but I am sure that none the less our hearts were with the veteran *savant*.

This year has also witnessed the celebration of the 250th anniversary of the University of Heidelberg. The Council had appointed our Foreign Secretary as a deputation to represent the Society on the occasion. Unfortunately when the time was close at hand, Dr. Williamson was prevented by the condition of his health from taking part in the celebration; but acting on the emergency on behalf of the Society, I requested our Fellow, Sir Henry Roscoe, to take his place, which he was so good as to do.

In his Presidential Address last year, Prof. Huxley suggested the idea, I may say expressed the hope, that the Royal Society might associate itself in some special way with all English-speaking men of science; that it might recognise their work in other ways than those afforded by the rare opportunities of election to our foreign membership, or the award of those medals which are open to persons of all nationalities alike. This suggestion has been taken up in one of our colonies. We have received a letter from the Royal Society of Victoria, referring to this passage in the Address, and expressing a hope that in some way means might be found for establishing some kind of connection between our own oldest scientific Society and those of the colonies.

The Council have appointed a Committee to take this letter into consideration, and try if they could devise some suitable plan for carrying out the object sought. The Committee endeavoured at first to frame a scheme which should not be confined to the colonies and dependencies of the British Empire, but should embrace all English-speaking communities. But closely connected as we are with the United States by blood and language, they are of course politically a foreign nation, and this fact threw difficulties in the way of framing at once a more extended scheme, so that the Committee confined themselves to the colonies and dependencies of our own country, leaving the wider object for some future endeavour, should the country concerned seem to desire it. The scheme suggested was laid before the members of the present Council, but there was not an adequate opportunity of discussing it, and it will of course come before the new Council. Should they approve of some such measures as those recommended by the Committee, they will doubtless assure themselves in some way or other that those measures are in accordance with the wishes of the Fellows at large before they are incorporated into the Statutes.

But in connection with this subject there is another suggestion which I would venture to offer, and which I know has been thought of by others.

A good many years ago it was not unusual to elect to the Fellowship men of distinguished eminence in departments other than scientific. More recently a change was made in the Statutes where y Privy Councillors are enabled to become Fellows by a special method, without interfering with the selection by the Council of fifteen from among the candidates whom they recommend to the Society for election. This to a certain extent superseded the necessity of appealing to other than scientific claims, and in some respects the method had special advantages. Those who attained to a place on Her Majesty's Privy Council were sure to be distinguished men, whom we should be glad to welcome among us; and by confining the privilege of special election to these, with whose appointment the Council had nothing to do, all invidious distinctions were prevented. But the method has the disadvantage that it applies only to a particular class of merit. A man, for instance, might be of quite first-rate eminence in poetry or literature, but that would not lead to a seat on the Privy Council. Such a man could only be elected by being placed on the selected list of fifteen. But it seems to me that there is something not quite courteous either to the eminent man himself, or to the scientific man who would have to be passed over to make room for him, in thus putting him in competition with those who seek admission on purely scientific grounds. I cannot help thinking that it might be well if the Council had the power of recommending for special election men of high distinction on other than scientific grounds, whose connection with us would on both sides be felt to be an honour, and who, though not, it may be, themselves scientific, might usefully assist us by their counsel. I do not think it would be difficult to devise means for providing that such a privilege should be accorded only in case of very high eminence.

The application of photography to the delineation of celestial objects has of late years made rapid strides; and, partly owing to the improved sensitiveness of the plates, partly to greater exactness in regulating the motions of equatorially-mounted telescopes, it has been found possible to photograph even minute stars. The question is accordingly now seriously entertained whether we may not trust to photography for the formation of star maps and star catalogues, taking eye-observations on a sufficient number of stars here and there for reference, and trusting to differential measurements taken on the plates for determining the positions of the other stars. Indeed, I think the practicability of this application may now be considered as established, and there only remains the question of the best mode of carrying it out on a uniform plan. In the course of the autumn I had a letter from Admiral Mouchez, Director of the Paris Observatory, in which he informed me that in response to the presentation of specimens of the admirable star photographs taken by the MM. Henry, several of the astronomers to whom they had been sent suggested that it would be well that a conference of astronomers of various nations should be held, with a view to taking concerted action for obtaining on a uniform plan a complete map of the whole starry heavens. He wished accordingly to obtain an expression of opinion on the part of the Royal Society as to the desirableness of holding such a conference; and as it was contemplated, in case the proposal should be favourably entertained by those consulted, that the conference should be held at Paris in the spring, and it would be necessary to give timely notice to the astronomers who live in the southern hemisphere, an early reply was requested.

As it would have defeated Admiral Mouchez's object to wait till the Council should re-assemble after the recess, I wrote at once to consult four of our Fellows specially named by Admiral Mouchez; and on receiving their replies I wrote to Admiral Mouchez, saying that under the circumstances I took it upon me to express in the name of the Royal Society our approval of the suggestion, explaining at the same time that I did so on the understanding, which I fully believed to be in accordance with his intention, that the astronomers who might attend the conference should not be considered as pledged to the adoption of the methods or scale of the MM. Henry, but that the whole subject should be open to discussion. On reporting what I had done to the Council when they met after the recess, I obtained an expression of their approval.

In these photographs a remarkable instance was exhibited of the power of photography to reveal the existence of objects wholly invisible to the eye. One of the stars of the Pleiades was found to be surrounded by a nebula which cannot be seen with telescopes. The reason of the difference of power of the plate and eye is very obvious: with the eye an object is either seen or not seen at once, whereas with the plate, provided there be an absence of stray light, feebleness of intensity can be made up for by length of exposure.

But the MM. Henry are by no means the only persons who have applied photography to the delineation of the stars. Among others, our Fellow, Dr. Gill, who has sent us some excellent specimens of the photographs obtained by his instrument, proposes to take at the Cape Observatory photographs of the whole starry heavens of the southern hemisphere, under such conditions as to include the magnitudes contained in Argelander's "Durchmusterung" of the northern hemisphere, and to subsequently reduce the observations so as to complete Argelander's great work by extending it to the southern hemisphere. Prof. Kapteyn, in Holland, has nobly undertaken to devote his spare time for seven years to superintending the reduction. Dr. Gill has laid the proposal before the Government Grant Committee. Having regard to the magnitude of the undertaking, and the probability of a conference of astronomers being shortly held in Paris to discuss the whole question, the Government Grant Committee suggested to the Council of the Royal Society that they should appoint a committee to take the subject into consideration, and the Council have acted on this suggestion. Dr. Gill intends to come to Europe in the spring, so that the committee will be able to consult him personally.

This morning I received through the Foreign Office an invitation from the Académie des Sciences for myself and some other delegate of the Royal Society to attend the conference to which I have already referred, which is fixed for April 16. I shall take the first opportunity of consulting the new Council as to their wishes.

The Copley Medal for this year has been awarded to the veteran in science, our Foreign Member, Prof. Franz Ernst Neumann, for his researches in theoretical optics and electrodynamics.

Having in his earlier years treated of crystallographic subjects almost half a century ago, he turned his attention to the theory of light. Fresnel had, with his wonderful sagacity, arrived at his celebrated laws of double refraction from the theory of transverse vibrations, aided by conceptions derived from a dynamical theory which was only in part rigorous. Cauchy and Neumann, independently of each other, were the first to deduce from a rigorous dynamical calculation, applied to a particular hypothesis as to the constitution of the ether, laws of double refraction, not indeed absolutely identical with those of Fresnel, but closely resembling them. In this case the laws were known beforehand. But in a very elaborate later paper, Prof. Neumann deduced from theory the laws of crystalline reflection, laws which appear to agree with the observations of Seebeck, and which had not been discovered by Fresnel, though some of them were independently and about simultaneously obtained by MacCullagh.

Prof. Neumann is perhaps still better known in connection with the theory of electro-dynamics, and the mathematical deduction of the laws of induced currents due to the motion of the primary and secondary conductor. He may be said to have completed for the induction of currents the mathematical treatment which Ampère had applied to their mechanical action.

Of the two Royal Medals, it is the usual, though not invariable, practice to award one for the mathematical and physical, and the other for the biological sciences.

One of these medals has this year been awarded to Prof. Peter Guthrie Tait, for his various mathematical and physical researches.

Prof. Tait is well known for his numerous and important papers in both pure mathemathics and physics. The late Sir William Hamilton regarded him as his own successor in carrying on and completing the newly-invented calculus of quaternions, of which Prof. Tait is continually making new applications. Among his investigations in the domain of experimental physics may be mentioned his determination of the conducting powers of metals for heat by a method which appears to possess special advantages, and his investigation of the effect of extremely great pressures on thermometers, undertaken with a view to deducing correct results for the temperatures at great depths in the ocean from the observations made in the *Challenger* expedition. This latter subject has led him to investigate the behaviour, as to compressibility and development of heat, of liquids and solids under enormous pressures, a subject in which he is still engaged. Before concluding, I must mention his elaborate papers on systems of knots, recently printed in the *Transactions* of the Royal Society of Edinburgh.

The other Royal Medal has been awarded to our Fellow, Mr. Francis Galton, for his statistical inquiries into biological phenomena.

Mr. Galton is well known as an explorer and geographer, and his mind is singularly fertile in the devising of ingenious instruments for various objects. Many years ago he brought before us some remarkable experiments instituted with a view to test a particular biological theory, in which rabbits of a pure variety were so connected with others of a different variety that the same blood circulated through both individuals, and the point to determine was whether this blood-relationship, in the most literal sense of the term, had any effect on the offspring. Contrary to what the theory in question led us to regard as the more probable, the result proved to be negative. It is, however, in accordance with the rules for the award of the Royal Medals, more especially the later investigations of Mr. Galton, in relation to vital statistics, that have been taken as the ground of the award. He has shown that by taking the average of a number of individuals having some condition in common, individual peculiarities apart from that condition are eliminated in the mean, and results are obtained which may be regarded as typical of that condition. One way of doing this is by his method of compound photographs. Thus we may obtain typical features of criminals of a particular kind, of consumptive persons, and so forth. By adhering to the method of averages, he has even succeeded in obtaining a mathematical expression, very closely verified in observation, connecting the mean deviation of some condition (such for example as stature) in a series of individuals, from the general mean of the same condition, with the mean deviations of the same condition in the relatives of those same individuals of different kinds, such as fathers, brothers, &c. Nor is the statistical method applicable to bodily characteristics alone. Mr. Galton has even extended it with remarkable ingenuity and originality to mental phenomena also.

The Rumford Medal has been awarded to Prof. Samuel P. Langley, for his researches on the spectrum by means of the bolometer.

A better knowledge of the ultra-red region of the spectrum, which includes the larger part of the energy of solar radiation, had long been a desideratum when Prof. Langley commenced his work upon this subject. Finding the thermopile insufficiently sensitive for his purpose, he contrived the "bolometer." This instrument depends upon the effect of temperature upon the electrical resistance of metals, a quantity susceptible of very accurate measurement; and, with its aid, Prof. Langley has been able to explore a part of the spectrum previously almost inaccessible to observation.

A result of Prof. Langley's work, very important from the point of view of optical theory and of the ultimate constitution of matter, relates to the law of dispersion, or the dependence of refrangibility on wave-length. Cauchy's formula, which corresponds well with observation over most of the visible spectrum, is found to break down entirely when applied to the extreme ultra-red.

Prof. Langley has given much attention to the important question of the influence of the atmosphere on solar radiation. The expedition to Mount Whitney, successfully conducted by him in face of many difficulties, has given results of the utmost value, pointing to conclusions of great interest and novelty.

The Davy Medal has been awarded to our Foreign Member, M. Jean Charles Galissard de Marignac, for his researches on atomic weights.

M. Marignac's numerous researches on atomic weights, which have been continued for a great number of years, have played an exceedingly important part in establishing and consolidating that ground-work of chemistry. They are remarkable for originality in devising methods appropriate to the respective cases, the most conscientious care in discovering errors which occurred in the respective operations, and indefatigable perseverance in finding means to eliminate the disturbing influences. His labours are all the more valuable because he chose for their field chiefly those elements which are most generally used in chemistry, and are most important to chemists, and on which the determination of new atomic weights is most generally made to depend.

TEN YEARS' PROGRESS IN ASTRONOMY[1]

III.

COMETS.—During the past ten years we have been favoured with an extraordinary number of comets, and while perhaps no single great step has been made, yet it is certain, I think, that our knowledge of these mysterious objects has gained a real and considerable advance.

In 1876, curiously enough, not a single comet appeared; but in 1877 there were 6; in 1878, 3; in 1879, 5; in 18 0, 5; in 1881, 8; in 1882, 3; in 1883, 2; in 1884, 3; and in 1885, 6; and so far this year, 3. Forty-four comets in all have been observed during the ten years, six of which were conspicuous objects to the naked eye, and two of them, the great comet of 1881, and the still greater one of 1882, were very remarkable ones.

The first of these will always be memorable as the first comet ever photographed. Dr. Henry Draper photographed both the comet itself and its spectrum; Janssen obtained a picture of the comet, and Huggins of its spectrum.

A number of excellent photographs were obtained of the great comet of 1882, especially by Gill, at the Cape. And it is worth mentioning that in May 1882 a little comet (not included in the preceding list, because no observations were obtained of it) was caught upon the photographs of the Egyptian eclipse.

Two of the bright comets, Wells's comet of 1881 and the great comet of 1882, approached very close to the sun, and their spectra, as a consequence, became very complex and interesting. A great number of bright lines made their appearance. Sodium was readily and certainly recognised; iron and calcium probably, but not so surely. The evidence as to the nature of the sun's corona, derived from the swift passage of the 1881 comet through the coronal regions, has already been alluded to.

The Pons-Brooks comet of 1883-84 is extremely interesting as presenting the first instance (excepting Halley's comet, of course) of one of the Neptunian family of comets returning to perihelion. There are six of these bodies with periods ranging from sixty-eight to seventy years. Halley's comet, the only large one of the group, has made many returns, and is due in 1910. Pons's comet, first observed in 1812, has now returned; Olbers's comet of 1815 is due in 1889, and the three others, all of them small, in 1919-20 and 1922.

I have spoken of them as Neptunian comets, *i.e.* their presence in our system is known to be due in some way to this planet. The now generally received theory is that they have had their orbits changed from parabolas into their present shape by the disturbing action of Neptune. Mr. Proctor has pointed out certain unquestionable, though, I think, inconclusive, objections to this view, and he proposes, as an alternative, the startling and apparently improbable hypothesis, that they have been *ejected* from the planet at some past time by something like volcanic action.

On the whole, however, the most important work relating to comets has been that of the Russian astronomer Bredichin. He has brought the mechanical and mathematical portion of the theory of comet's tails to a high degree of perfection; following out the lines laid down by Bessel, but improving and correcting Bessel's formulæ, and determining their constants by a most thorough discussion of all the accurate observations available.

It is hardly possible to doubt any longer that all the facts can be represented on the hypothesis that the tails are composed of minute particles of matter, first driven off by the comet, and then repelled by the sun. Bredichin's most interesting result, arrived at in 1878, is that the tails appear to be of three, and only three, distinct types—the long straight streamers which are due to a repulsive acceleration about twelve times as great as the sun's attraction; the second and most ordinary class of broad-curved tails for which the repulsive force ranges between one and two and a half times that of the attraction; and, finally, the short, stubby brushes which are found in a few cases, and correspond to a repulsive force not more than one-fourth the sun's attraction. Supposing, as he does, that the *real* repulsion is the same for each atom, the *apparent* repulsion, or repulsive *acceleration*, would be greater for the lighter atoms, and nearly inversely proportional to their molecular weights; and so he concludes that probably tails of the first type are composed of hydrogen, those of the second type of hydrocarbons, like coal-gas, and those of the third of iron, and its kindred metals. As to the second type, the spectroscope speaks distinctly in confirmation. Tails of the first and third types are not common, and are usually faint, and since Bredichin's result was announced there has been no opportunity for spectroscopic verification in their case.

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[1] "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young. Read May 17, 1886, before the New York Academy of Sciences. Continued from p. 98.

I said his investigations had given a mathematical and mechanical explanation of comets' tails; but the *physical* question, as to the nature of the force which causes the observed repulsion, remains unsettled, though I think there is no doubt that general opinion is crystallising into a settled belief that it is electrical; that the sun is not at the same electric potential as surrounding space, and that, in consequence, semi-conducting masses of pulverulent matter, such as comets seem to be, are subject to powerful electric forces as they approach and recede from the central body. At the same time there are those—Mr. Ranyard, for instance—who forcibly urge that the direct action of the solar *heat* might produce a similar repulsive effect by causing rapid evaporation from the front surface of minute particles, charged with gases and vapours, *frozen* by the cold of outer space.

I ought not to dismiss the subject of comets without at least alluding to the numerous unprecedented and interesting phenomena presented by the great comet of 1882: first, its unquestionable relation to, but distinctness from, its predecessors of 1880, 1843, and 1668, the three belonging to one brotherhood, of common origin, and all following nearly the same path around the sun. I call special attention to this point, because Miss Clerke, in her new and admirable "History of Astronomy in the Nineteenth Century" (which I hope every one interested in astronomy will read as soon as may be), has, I think, made a mistake regarding it, assigning to the difference between the computed periods of these comets much too great an importance.

The strange elongation of the nucleus of this comet into a string of luminous pearls; the faint straight-edged beam of light that enveloped and accompanied the comet for some time; and the several detached wisps of attendant nebulosity that were seen by several observers, are all important and novel items of cometary history.

Meteors.—Time will not allow any full discussion of the progress of meteoric astronomy. It must suffice to say that the whole course of things has been to give increased certainty to our newly-acquired knowledge of the connection between meteor-swarms and comets, and to make it more than probable that a meteor-swarm is the result of the disintegration and breaking-up of a comet. This seems to be the special lesson of the Bielids, the re-appearance of which, as a brilliant star-shower last November, attracted so much attention. In an important paper read before the National Academy of Sciences, last April, Prof. Newton pointed out how all the facts connected with the division into two of Biela's comet forty years ago, its subsequent movements and disappearance, and the meteoric showers of 1872 and 1885, and especially the peculiar features of this last shower, all conspire to enforce this doctrine.

I mention, doubtfully, in this same connection the recent supposed discovery by Denning of what are generally alluded to as "long radiants": systems of meteors, *i.e.*, which for weeks, and even months, together, seem nightly to emanate from the same point in the sky. One of these radiants, for instance, the first of half a dozen described by Mr. Denning, is about 1½° north of β Trianguli, and the shower appears to last from July to November, at the rate of perhaps one or two an hour.

If the fact is *real*, it follows inevitably that, disseminated through all the space in which the earth is moving, and has been moving for several years—not less than 1,000,000,000 miles—there are countless meteoroids moving in parallel lines, and with a velocity so great that the earth's orbital motion of 19 miles a second is absolutely insignificant as compared with theirs. Their speed must be many hundreds of miles per second. This may be true, but I own I am not ready to accept it yet. The observations indicate directly no extraordinary swiftness. Mr. Proctor, whose mind appears at present to be chiefly occupied by the idea that suns and planets are continually bombarding their neighbours (or at least do so at some stage of their existence), ascribes such meteors to the projectile energies of some of the "great" stars. But there is not time to discuss his notion, and it is hardly necessary, until it has begun to receive somewhat more extensive acceptance. I am not aware that, so far, he has any converts to his theory of comets and meteors.

Stars.—Want of time will also prevent any adequate treatment of the recent progress of stellar astronomy.

Two great works in the determination of star places must, however, be mentioned. One is the nearly completed catalogue of all the northern stars, down to the ninth magnitude, begun almost twenty years ago, under the auspices of the Astronomische Gesellschaft, by the co-operation of some fifteen different observatories. The observations are now nearly finished, and several of the observatories have already reduced and published their work. A very few years more ought to bring the undertaking to a successful end.

Another similar work, almost, though not quite, as extensive, is the great catalogue of southern stars, made at the Observatory of Cordova by our own Dr. Gould and his assistants. He himself, with his own eyes, observed every star of the whole number—nearly 80,000—his assistants reading the circle and making the records; and the whole has been reduced, printed, and published within the space of twelve years—a veritable labour of Hercules, for which, most justly, our National Academy has awarded him the Watson Medal. He had already, some years ago, received the gold medal of the English Royal Astronomical Society, for the "Uranometria Argentina," an enumeration of all the naked-eye stars of the southern hemisphere, with their approximate positions and estimated magnitudes. This, however, was only a sort of preliminary by-play, to pass the time while waiting for the completion of his observatory and meridian-circle.

We must mention also the remarkable star-charts made by Dr. Peters, of Hamilton College, of which he has already published and distributed at his own expense about twenty, and more are soon to follow.

But the old-fashioned way of cataloguing and charting the stars is obviously inadequate to the present needs of astronomy, and a new era has begun. While, hereafter, as hitherto, the principal stars, several hundred of them, will be observed even more assiduously and carefully than ever before, with the meridian-circle or similar instruments, the photographic plate will supersede the eye for all the rest. It is now easily possible to photograph stars down to the thirteenth or fourteenth magnitude, and to cover a space of 2½° square on a single plate. The remarkable thirteen-and-a-half-inch instrument constructed by the Henry Brothers, for the Paris Observatory, and first brought into use last August, does this perfectly. Instruments very similar, but smaller, lately set up at Harvard College, at the Cape of Good Hope, and at Liverpool, while they do not reach so faint stars, cover more ground at a time.

Negotiations are already under way to secure the co-operation of a number of observatories for a photographic survey of the heavens; and it is probable that, after some preliminary consultation and before very long, it will be actually in progress. According to Struve's estimates, it could be accomplished in about ten or twelve years, even on the Paris scale, by the combined efforts of fourteen or fifteen establishments. Orders have already been given to the Henry Brothers, by Dom Pedro, of Brazil, and Mr. Common, of England, for instruments precisely like the one at Paris. Americans, and New Yorkers especially, may well take a peculiar interest in astronomical photography, since it was at Cambridge, in 1861, that the first star-photographs were ever made, and here, in New York, Rutherfurd and Draper were among the earliest and most successful workers: in the observatory above us is now mounted the very instrument with which Rutherfurd made his unrivalled pictures of the moon and his plates of the Pleiades, more than twenty years ago.

During the past ten years, stellar photometry has become almost a new science. Its foundations, indeed, were laid by J. Herschel, Seidel, Wolff, and Zöllner, before 1870, and the magnitudes of some two hundred stars had been measured, and the law of atmospheric absorption determined. But the great work of Pickering, at Harvard, in the invention and perfecting of new instruments, and his "Harvard Photometry," which gives us a careful measurement of the brightness of all the naked-eye stars of the northern hemisphere, marks an epoch. And he is pushing on, and has already well under way the measurement of the 300,000 stars of Argelander's "Durchmusterung." Nor must we omit to mention Pritchard, of England, whose name has just been joined with Pickering's by the Royal Astronomical Society in the bestowal of their gold medal for his wedge-photometer and the photometric work done with it. The "Harvard Photometry," and the "Uranometria Oxoniensis" together will carry down to all time the record of the present brightness of the stars. They will be especially valuable as data for determining changes in stellar brilliancy.

During the past ten years the number of variable stars has risen from about 100 to nearly 150; and our knowledge of their periods and light-curves has been greatly improved. In America, Chandler and Sawyer, of Boston, and Parkhurst, of this city, have done especially faithful work. During the ten years we have had two remarkable "temporary stars," as they are called—first the one which, in November 1876, in the constellation of Cygnus, blazed up from the ninth magnitude to the second, and then slowly faded back to its former brightness, but to a *nebulous* condition, as shown by its spectrum; then also the one which, last autumn, appeared in the heart of the nebula of Andromeda as of the sixth magnitude (where no star had ever been seen before), slowly dwindled away, and is now beyond the reach of any existing telescope. Perhaps, too, we ought to mention another little ninth-magnitude star in Orion's club, which last December rose to the sixth magnitude, and is now fading; it seems likely, however, from its spectrum, that this is only a new variable of long period.

As to star-spectra, a good deal of work has been done in their investigation with the ordinary stellar spectroscopes by the Greenwich Observatory, by Vogel at Potsdam, and by a number of other observers,—work well deserving extended notice if time permitted. But the application of photography to their study, first by Henry Draper in this city, and by Huggins in England, is the important new step. By the liberality of Mrs. Draper, and as a memorial of her husband, his work is to be carried on with the new photographic instrument and method just introduced by Prof. Pickering at Cambridge. He is able to obtain on a single plate the spectra of all the stars down to the eighth magnitude in the group of the Hyades, each spectrum showing under the microscope the characteristic lines quite sufficiently for classification. A different instrument is also to be built with the Draper Fund, which will give single star-spectra on a much larger scale and in fuller detail.

During the decade, the stellar parallax has been worked at by a number of observers. Old results have been confirmed or corrected, and the number of stars whose parallax is fairly determined has been more than doubled. The work of Brunnow and Ball in Ireland, of Gill and Elkins at the Cape of Good Hope, and of Hall, at Washington, deserves especial mention. A new heliometer of seven inches aperture has been ordered for the Cape Observatory, and when it is received, a vigorous attack is planned by co-operation between that observatory and that of Yale College, which possesses the only heliometer in America.

During the ten years, our knowledge of double stars has been greatly extended; several observers, and most eminent among them Burnham, of Chicago, have spent much time as hunters of these objects, and have bagged between one and two thousand of them. Several others, especially Doberck in England, and Flammarion in France, have devoted attention to the calculation of the orbits of the binaries, so that we have now probably about seventy-five fairly well defined.

In the study of the nebulæ, less has been done. Stephan at Marseilles and Swift at Rochester have discovered many new ones, mostly faint, and Dreyer, of Dublin, has published a supplementary catalogue, which brings Sir J. Herschel's invaluable catalogue pretty well down to date. The studies of Holden upon the great Orion nebula and the so-called "trifid nebula" deserve special mention, as securely establishing the fact that these objects are by no means changeless, even for so short a time as twenty or thirty years; also the discovery of a new nebula in the Pleiades by means of photography.

Observatories.—During the ten years, a considerable number of new observatories have been founded. Abroad, we mention as most important the observatories for astronomical physics at Potsdam, in Prussia, and at Meudon, in France, also the Bischoffsheim Observatory at Nice and its succursal in Algiers. The great observatory at Strasburg can hardly be said to have been founded within the period indicated, but the new buildings and new intruments and new efficiency date since 1880. We ought not to pass unnoticed the smaller observatory at Natal, in South Africa, and the private establishments of von Konkoly at O-Gyalla, of Gothard at Hereny (both in Hungary), and of the unpronounceable gentleman Jedrzejewicz at Plonsk, in Poland, and the observatory at Mount Etna, from which, however, we have no results as yet.

In the United States, we have the public observatories at Madison, Wis., at Rochester, N.Y., and at the University of Virginia, and the, as yet, unfinished Lick Observatory in California: also a host of minor observatories connected with institutions of learning, and mainly designed for purposes of instruction; such establishments have been founded within ten years at Princeton, at Northfield, Minn., at South Hadley, Ms., at Beloit, at Marietta, at Depauw, at Nashville, and at St. Louis, also at Franklin and Marshall College, and at Doane College, in Nebraska, at Columbia College, Ann Arbor, and Madison, Wis., and at one or two other institutions which escape me for the moment. Several others are also at this moment in process of erection. Every one of them has a telescope from six to thirteen inches aperture, with accessory apparatus sufficient, in the hands of an astronomer, for useful scientific work.

Instruments.—A large number of new instruments of great power have been constructed. We mention the great thirty-inch refractor of Pulkowa, the twenty-six-inch of Charlottesville, and the twenty-three inch at Princeton, for all which the lenses were made by our own Clark. We add the great Vienna twenty-seven-inch telescope by Grubb, and the twenty-nine-inch object-glass by the Henrys, made for the Nice Observatory, but not yet mounted; also the nineteen-inch telescope at Strasburg by Merz. Grubb has also at present a twenty-eight-inch object-glass under way for the Greenwich Observatory, and Clark has nearly completed the monstrous thirty-six-inch lens for the Lick Observatory. There never was a decade before when such an advance in optical power has been made.

Great *reflectors* have been scarce, the only ones of much importance constructed during the time being the twenty-inch instrument at Algiers, and Mr. Common's exquisite three-foot telescope, which he has lately sold to Mr. Crossley in order to make way for one of five feet diameter, now, I believe, under construction. The old three-foot and six-foot instruments of Lord Rosse have been improved in various ways, and are still in use,—especially in work upon lunar heat. Among newly-*invented* instruments, we mention the meridian-photometer of Pickering, the wedge-photometer of Pritchard, the almucantar of Chandler, the concave diffraction-grating of Rowland, and the bolometer of Langley—all, but one, American. Repsold's improvements in the micrometer, in the heliometer, and in the mounting of equatorials should also be mentioned here.

As to new astronomical methods, enough has been already said about photometry and astronomical photography. It is plain that we are entering upon a new era.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE examination for the Sheridan Muspratt Chemical Scholarship at University College, Liverpool, will begin on December 9. The Scholarship is of the value of 50*l.* per annum, tenable for two years. Candidates should apply to the Registrar before December 6.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, November 22.—M. Jurien de la Gravière, President, in the chair.—On the life and work of L. R. Tulasne, by M. Ed. Bornet. The paper contains a somewhat detailed account of the labours of this eminent botanist, who was born at Azay-le-Rideau (Indre-et-Loire) on September 12, 1815, and died on December 2, 1885. Appended is a list of the scientific publications of MM. Louis-René and Charles Tulasne.—On ammoniaco-magnesian phosphate, by M. Berthelot. In continuation of his previous researches on the colloidal and crystallised states of the earthy phosphates, and especially of the phosphate of magnesia, the author here studies the double ammoniaco-magnesian phosphate, determining the conditions of formation of this compound in chemical analysis.—The Montgaudier Cave, by M. Albert Gaudry. The author describes a visit he recently paid to this cave, which is situated in the Charente district, and which has revealed several objects of an artistic character, dating from the close of the Quaternary epoch, when the large fauna of extinct species had already mostly disappeared. But some remains, such as pieces of ivory embellished with carvings of the aurochs and other animals, seem to have been executed at an earlier period, while the cave-dwellers were still struggling for existence with the mammoth, with *Rhinoceros tichorhinus*, the cave-bear, cave-lion, and large hyæna.—On glycose, glycogene, and glycogeny, in their relation to the production of heat and of mechanical labour in the animal system, by M. A. Chauveau. In this first physiological study of these elements, the author deals more especially with the generation of heat in the organism while in a state of repose. The reasons are set forth which lead to his broad generalisation regarding the preponderating part played by the glycose of the blood in organic combustions, source of animal heat and of muscular energy. It is now fully established that the absorption of glycose in the capillaries during the transformation of arterial into veinous blood is connected with the respective activity of the attendant combustions in the several organs.—Some remarks on the determination of mean values, by Leopold Kronecker. It is shown that in a converging series of real terms $\phi_1 + \phi_2 + \phi_3 + \ldots$ with positive real quantities $\psi_1, \psi_2, \psi_3 \ldots$ increasing with n beyond all limit, the limit of the expression—

$$\frac{1}{\psi_n}(\phi_1\psi_1 + \phi_2\psi_2 + \phi_3\psi_3 + \ldots + \phi_n\psi_n)$$

for increasing values of n is equal to zero.—On the movement of an indefinite and perfectly elastic fluid, by M. N. Marin. The object of this study is to complete the law of Mariotte by another described as the law of elasticity for perfectly elastic and completely free fluids. In a fluid so constituted, it is laid down that all contraction determined by any cause whatsoever acting in a single direction, is instantaneously propagated in all other directions.—On the movement of a cord in a fixed plane, by M. Appell.—On the algebraic integrals of Kummer's equation, by M. E. Goursat.—Analytical demonstration of a theorem relating to orthogonal surfaces, by M. Paul Adam. The theorem here dealt with is that of M. Maurice Lévy regarding a group of surfaces in an orthogonal system, and by him demonstrated on purely geometrical considerations.—On the unequal movement of a compressed gas in a reservoir freely discharging into the atmosphere, by M. Hugoniot.—On an apparatus by which the time may be communicated to the performers out of the conductor's sight, by M. J. Carpentier. The apparatus here described has been constructed at the request of the directors of the Paris Opera. It is based on the principle of visible signs, depending on a purely optical illusion, and producing the impression of an ordinary conductor's hand beating time. It is thus free from the defects inherent to the various electric appliances hitherto devised for the same purpose.—On a means of increasing the power of fluid and electric agencies, by M. Charles Cros. In this process a return is made to the old idea involved in the expression "electric fluid," and the wires are regarded as analogous to elongated tubes through which pressure is transmitted. The experiment was suggested by the author's researches on transmissions through more or less elastic tubes containing air or water.—On the tension of saturated vapour, by M. P. Duhem.—On the physical properties of mercury, by M. Marcellin Langlois. On the assumption of a mono-atomic molecule of mercury, the author determines its heat of evaporation, its specific heat, compressibility, and heat of fusion.—Actinometric studies, by M. E. Duclaux.—A new process of volumetric analysis for powdery zinc (*gris d'ardoise* of la Vieille Montagne), by M. Fréderic Weil. By the process here described, 100 gr. of this substance yield 65·3 gr. of pure zinc.—Action of the alcohols on the protochloride of gold and phosphorus, by M. L. Lindet. Here are described the preparation and properties of two chlauroso-phosphorous ethers—ethylic and methylic ether.—On the Russian petroleums, by M. J. A. Le Bel. The chief element of the petroleum of Baku, at the eastern extremity of the Caucasus, are naphthenes, C_nH_{2n}, and naphthylenes, C_nH_{2n-2}; and their salient characteristic is that they do not fix bromine.—On the heats of neutralisation of malic and citric acids and of their pyrogened derivatives: remarks on the numbers obtained, by MM. H. Gal and E. Werner.—On certain correlations between the modifications experienced by species of different genera subjected to the same influences, by M. Fontannes. Several analogous modifications are noted pervading many species of different genera throughout the geological record; but no theory is advanced to explain the coincidences.—On a new genus of parasitic Copepod, by M. Eugène Canu. This new genus is a parasite of the Synascidians, and abounds on the *Morchellium argus* (Milne-Edwards) frequenting the Wimereux district.—On the anomalous formations of the Menispermeæ, by M. Gérard.—Observations on the plaster added to new wines in the South of France and other parts of Europe, by M. A. Audoynaud.—Note on the coarse marine limestone formation of the Provins district (Seine-et-Marne), by M. Stanislas Meunier.—On the Devonian system of the Eastern Pyrenees range, by M. Ch. Depéret.—On the pleromorphoses of the quartz of Saint-Clément (Puy-de-Dôme), by M. Ferdinand Gonnard.—Description of a variety of Carphosiderite, by M. A. Lacroix. The optical properties of this mineral, which was found in the neighbourhood of Mâcon (Saône-et-Loire), are described.—On the conditions of form and density of the terrestrial crust, by M. A. de Lapparent. It is argued that the generally-accepted views regarding the symmetrical flattening of the globe at the poles is far from proved, and it is suggested that in the southern hemisphere there exists an inaccessible antarctic continent presenting a different conformation in this respect from that of the northern hemisphere.—On the mode of formation of the Newfoundland banks, by M. J. Thoulet.—On the progressive desiccation of lacustrine basins in dry climates, by M. Venukoff.

BOOKS AND PAMPHLETS RECEIVED

Die Schiffsmachine; Atlas: Bushey (Lipsius and Tischer, Kiel).—Second Armagh Catalogue of 3300 Stars: Robinson and Dreyer (Thom, Dublin).—An Arctic Province; Alaska and the Seal Islands: H. W. Elliot (Low).—Wild Animals Photographed and Described: J. F. Nott (Low).—Quarterly Journal of the Geological Society, vol. xlii. part 4, No. 168 (Longmans).—Studies from the Biological Laboratory, Johns Hopkins University, vol. iii. No. 8.—Calendar of University College of South Wales, 1886-87 (Owen, Cardiff).—Géologie de Jersey: Le P. C. Noury (Sary, Paris).—Memoirs of the Geological Survey of India—Palæontologia Indica, ser. x.; Indian Tertiary and Post-Tertiary Vertebrata, vol. iv. part 2, The Fauna of the Karnul Caves: R. Lydekker (Trübner).—Descriptive Catalogue of a Collection of the Economic Minerals of Canada: A. R. C. Selwyn (Alabaster).—L'Égalité des Sexes en Angleterre: F. Remo (Nouvelle Revue, Paris).—Theory of Magnetic Measurements: F. E. Nipher (Van Nostrand, New York).—Outlines of the Geology of Northumberland and Durham: G. A. Lebour (Lambert, Newcastle-on-Tyne).—Lehrbuch der Histologie: Dr. P. Stohr (Fischer, Jena).—Lehrbuch der Entwicklungsgeschichte, Erste Abth.: Dr. O. Hertwig (Fischer, Jena).—Lunar Science, Ancient and Modern: Rev. T. Harley (Sonnenschein).—Hourly Readings 1884, part 1, January to March.—The Auk, October, vol. iii., No. 4 (New York).—Journal of Physiology, vol. vii., Nos. 5 and 6 (Cambridge).—Notes from the Leyden Museum, Nos. 1 to 4, 1886 (Brill, Leyden).—Observations Nouvelles sur le Tufeau de Ciply: A. Rutot and E. van den Broeck (Liége).—Proceedings of the Academy of Natural Sciences of Philadelphia, April to September 1886 (Philadelphia).—The Washoe Rocks: G. F. Becker.—On the Origin of Agriculture: H. Ling Roth (Harrison).

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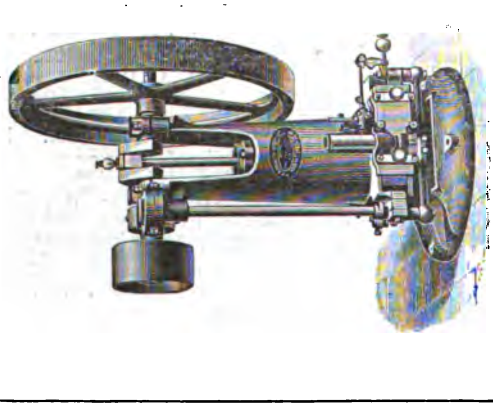

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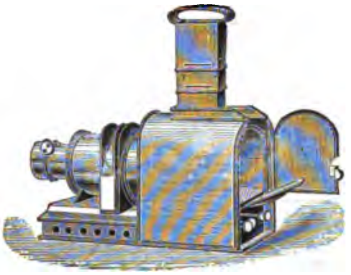

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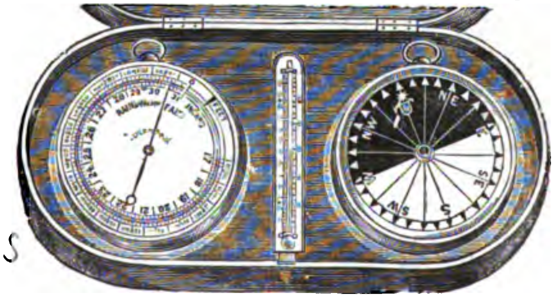

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COMPARATIVE ANATOMY OF VERTEBRATES

Elements of Comparative Anatomy of Vertebrates. Adapted from the German of Robert Wiedersheim, by W. Newton Parker. (London: Macmillan and Co., 1886.)

IN examinations for the higher degrees and diplomas in science and medicine, candidates are required to show that they possess not only a knowledge of the anatomy of the chief types of the animal kingdom, but also of comparative morphology. Indeed, with respect to medicine, this latter is the more important, especially the morphology of vertebrates. For some time past English students have found themselves considerably handicapped by the want of a short and concise text-book on this subject, to enable them to meet the requirements of Examination Boards; the text-books available for their perusal being generally of too advanced a character, and better suited for the use of those wishing to make comparative anatomy a lifelong study, than for students whose ulterior aim is the practice of medicine or some of its branches. German students, notwithstanding the numerous works on comparative anatomy published in that country, seem to have been equally as ill off for a suitable text-book as their English *confrères*. So impressed was Prof. Wiedersheim that his "Lehrbuch der Vergleichenden Anatomie," a work well known and appreciated by comparative anatomists in this country, and one of the leading works on the subject in Germany, was not a suitable book for ordinary students of medicine, that before bringing out a new edition of it he published a smaller one, entitled "Grundriss der Vergleichenden Anatomie der Wirbelthiere," expressly to meet their requirements. The number of English students sufficiently acquainted with German to be able to take advantage of this work in the original is unfortunately very small, but to those who could do so it has proved to be of great assistance. So well has the German edition fulfilled the object of its author, that the idea occurred to others besides Mr. Parker, that the translation of so useful a work into English was very desirable in the interests of English-speaking students, though it cannot but be a matter of regret that this should be necessary, and probably would not have been so, were English students as well acquainted with the German language as its importance demands they should be.

Throughout the work before us Mr. Parker has retained the original plan of Prof. Wiedersheim's "Grundriss," but a considerable number of additions to and modifications of the original text have been introduced. For some of these he acknowledges that he is responsible, while others have been inserted on the suggestion of Prof. Wiedersheim, who has also revised the whole work previously to its publication.

The work begins with a general introduction, in which the nature and meaning of comparative anatomy is explained, a short outline of the embryological development and structural plan of the vertebrate body is sketched out, and a table is given containing the general classification adopted by the authors throughout the work, of the principal existing vertebrate groups; there is likewise a second table from H. Credner, showing the gradual development of the Vertebrata in time. This part of the work occupies only 15 pages, and is well illustrated by means of nine woodcuts. It is preceded by a list of general works on comparative anatomy and embryology.

The comparative anatomy of vertebrates, constituting the special part of the work, and occupying 315 pages—the remainder of the book—is dealt with under nine sections, arranged according to the different organs of the body. These are treated of in the following consecutive order: integument, skeleton, muscular system, electrical organs, nervous system, organs of nutrition, organs of respiration, organs of circulation, and urinogenital organs. This part of the work is illustrated by 320 woodcuts, most of which are taken from the German edition, but some are new. After each section is appended a short bibliographical list of the more important and recent works relating to the subject under consideration in the chapter.

In treating each section the plan adopted is to begin with a few general introductory remarks, applicable to the whole of the Vertebrata, on the anatomy of the organ or set of organs to be dealt with in the section, and then to proceed to the consideration of its special characters in the different groups of vertebrates. By two kinds of type being used, one larger than the other, the more essential characters of organs or structures are readily distinguishable by the elementary student from the more theoretical and detailed information regarding them printed in the smaller type. The more important words and passages are further indicated by the use of heavy black and spaced types. The former we think might well have been dispensed with, as the use of so many kinds of types tends rather to confuse than render clear. The use of spaced type for important passages and words, though extensively used in German text-books, is only just beginning to be adopted by English authors and printers in place of italics, on which it is a very great improvement. We must, however, enter a decided protest against the use of clarendon type for any purpose except headings, on account of the blotted and disfigured appearance it gives to the pages and on account of the distraction it produces on the reader. Notes explanatory of the text are frequently added at the foot of the page throughout the whole work; that this should have been done is, we consider, a mistake. The introduction of footnotes into any work, and particularly such a work as the present, is extremely objectionable, and should be resorted to as seldom as possible except for references to other publications, where its use may be permissible. Nor is there any excuse in the present instance why so many footnotes should have been introduced, when by the use of a few lines of the smaller type in the body of the page it could have been avoided.

Throughout the text there is a want of uniformity in the manner in which the different kinds of type are used, which will probably be corrected in a new edition; thus, for example, on p. 49 the word "Snakes" is printed in spaced type, whereas three lines lower down "Lizards," another group of Reptiles, is printed in ordinary type, as are also the words "Birds" and "Mammals" on the next page. There are also some descriptions of structures which might be improved in another edition, so as to make the meaning clearer to the student; as an example of this, the history of the development of hair may be mentioned. The brevity of some of the descriptions is, as the authors state, to some extent made up for by the number of woodcuts, but in some cases we think it would have been well had a few lines more been added, even had it been necessary to curtail some of the small print; thus on the first page surely it would have been well to have devoted a little more space to explaining what is meant by the words "ontogeny" and "phylogeny," terms which are constantly referred to throughout the work. It is true that this is also a fault of the original German edition, but as the translator has not professed to adhere strictly to the German text this is a liberty which he might have taken.

We regret to see the words ecto-, meso-, and endoderm used by Prof. Wiedersheim in the original German changed to epi-, meso-, and hypoblast in the English edition, as we consider the terms used by German and other Continental morphologists preferable to those employed by many English writers. We also traverse the statement in the footnote, presumably Mr. Parker's, that the former terms "are applied to the corresponding layers in the adult animal." The terms are respectively synonymous when applied to vertebrates; the skeleton or muscular system of a rabbit is not spoken of as its mesoderm, but as being of mesodermic or mesoblastic origin.

In describing the homologies of the carpus and tarsus, we quite approve of the position the authors have taken with regard to a subject of much controversy on which further investigations may throw more light, but we think in the general considerations regarding the derivation of limbs from fins the history of the evolutionary changes which have taken place is stated much more definitely than the state of our knowledge justifies. That the theory with which Prof. Wiedersheim's name is connected should be brought prominently forward was to have been expected, but no reference is made to other views, such as those of Gegenbaur and Götte, which, in the uncertain state of our knowledge of the subject, might have been expected. We notice that the pentadactyle form of *Equus* found by Marsh has been omitted in the illustrations of the ancestral forms of the horse's foot, and referred to only doubtfully in the text.

In the section on organs of circulation, the heart and its vessels are excellently described, and the introduction occasionally of coloured illustrations makes the subject as clear as could be desired, but the description of the formation of the circulation in the liver and the modifications of the trunk vessels which have occurred in the evolution of the higher from the lower vertebrates, is very short and meagre.

The criticisms which we have passed, however, are not made with the view of finding fault with an excellent and creditable work, but are expressed in the hope that in future editions it may be made more useful for the purpose for which it is intended.

In conclusion, we heartily thank Mr. Parker for introducing to English students a work which we are confident will prove a great assistance to them in their studies, and by perusal of which they will be enabled to understand the anatomy of man in a much more comprehensive manner than they could from a study of human anatomy alone.

J. G. G.

SCIENCE IN NORWAY

Nyt Magazin for Naturvidenskaberne. "New Journal of Natural Science." (Christiania, 1886.)

THE four volumes of this admirable publication which have been sent to us by the Norwegian publishers, include the twelve numbers printed since 1882, and thus complete the third series of the magazine, which, notwithstanding its title of "New," is the oldest Norwegian publication of its kind.

The subjects treated of in its pages belong, with few exceptions, to the general natural history of Norway, in which department special attention is due to Herr Leonard Stejneger's paper on the "Ornithology of Western Norway," as well as to the various interesting contributions of Dr. Robert Collett. To the latter eminent naturalist, well known for his able work "On the Fishes of Norway" (1874), the magazine is indebted for several communications regarding the number and distribution of various species of fishes, first observed in Norway between 1879 and 1883, while in his papers on the character and species of mammals indigenous to Norway, he has contributed much useful information in reference to the Norwegian beaver (*Castor fiber*). From the author's observations, it would appear that the injury done to the forests by these animals in bringing down trees for the construction of their huts in no way justifies the indiscriminate destruction of the beaver, which had long been allowed to go on unchecked in Norway. This view has of late been so generally accepted that the Norwegian Government have been induced to appoint a close time of nine months in the year, during which the shooting or trapping of the beaver is legally prohibited. In special cases, and in certain districts, the local magistrates may even extend this period to ten years if they see reason to apprehend the extermination of the animal.

Norwegian geology and mineralogy are well represented in these volumes, Dr. Kjerulf having added to his labours of an editor those of a diligent contributor, and besides several short papers on the finer mineralogical specimens derived from the Storvarts and other important Norwegian mines—a subject which is also well treated by Herrn Olsen, Münster, and Knudsen—he has enriched the journal with interesting monographs on the local geology of the country, including notices of the dislocations observable in the Christiania valley; the character of the formations at Mjösen-lake which have been brought to light through recent railway-cuttings; and the inclination of the principal lodes on Ekersund. Dr. Hans Reusch describes at length the fall of meteors observed at Vaage in Tysne Island on May 20, 1884, giving useful tables of meteoric falls, whose dates are well attested, between 1784 and 1884. Besides various other papers, Dr. Reusch contributes some interesting geological notices of the districts of Christiania, Valders, and Viksnes, and considers at some length the evidence afforded by the fjelds of Western Norway, of the duration of the Ice age, and the local extension of glacial action in Norway.

The marine zoologist will find much valuable matter in the interesting reports by Messrs. Danielssen and Koren of the Asteridea, collected in the Norwegian North Atlantic Expedition. These papers are a *résumé* of the complete volume, which will appear later on as part of the Collective Report of the Expedition. Of the 20 genera and 40 species collected, 4 genera and 11 species are new to science. Numerous specimens of the Pedicellaster found in West Greenland, and described by Dr. Sladen as new, to which he gave the specific name of Palæocrystallus, is identified by Dr. Danielssen as *P. typicus* of Sars. Extreme importance attaches to the discovery and careful examination of a specimen of an asteroid—unfortunately the only one secured—which differs from others of the family by having a central dorsal appendage, generally erect, but capable of motion. This curious Echinoderm, to which Messrs. Danielssen and Koren give the name *Hyaster mirabilis*, is conjectured by them to represent a larval or developmental stage of the Crinoidea, and after a careful study of this stalk-like appendage they hazard the conjecture that further investigations may lead to the discovery that the Asteridea are in fact developed from the Crinoidea. Equally interesting, if less important, is the re-discovery of the Greenland "Cluster-polyp" of Ellis, the "*Zoophytum grönlandicum*" of Mylius. The specimens examined by these earlier naturalists have long disappeared, and for more than a hundred years no others were found. The *Challenger* Expedition brought back several forms of an Umbellula, one of which Prof. Kölliker considered to be of the same species as the lost specimens of Ellis and Mylius; the Norwegian naturalists are of opinion, however, that all the specimens found are mere varieties of *Umbellula encrinus*, to which they ascribe a wide geographical range.

In conclusion, while we desire strongly to recommend the *Nyt Magazin*, it may not be out of place to mention that several of the most interesting papers on local Norwegian geology are written in German, and that the highly important results of the recent Norwegian North Atlantic dredgings are given by Dr. Danielssen in English, under the title of a "Preliminary Report" of the Expedition. The magazine, which is under the joint editorship of Professors Kjerulf, Danielssen, Mohn, and Hiortdahl, is printed in the Latin type now so generally used in the Norwegian press, and is copiously illustrated by well-drawn woodcuts, and excellent plates of the animals described.

OUR BOOK SHELF

Acta Mathematica. (Stockholm. Various dates.)

THIS journal, which has already won for itself the reputation of being one of the leading mathematical journals, not of the North merely, but of the world, sprang into life at the end of 1882, is published at Stockholm, and has all along been under the able editorship of Prof. G. Mittag-Leffler, assisted by all the foremost mathematicians of Sweden, Norway, Denmark, and Finland. Its object is stated to be to gather and publish such mathematical works as contribute to the development of the science by the novelty either of the results obtained or of the methods employed.

The seven volumes which have been issued contain papers by some of the foremost Continental mathematicians: the sole contribution, we believe, in English is furnished by an American writer, Mr. G. W. Hill, and is entitled "On the Part of the Motion of the Lunar Perigee which is a Function of the Mean Motion of the Sun and Moon" (this paper occurs in vol. viii., which is in course of publication). There are in all, in the complete volumes, 107 papers, in almost every department of the science.

It may be in the recollection of our readers that Oscar II., King of Sweden and Norway, who is styled "Special Protector of the Journal," has instituted a great mathematical prize for an important discovery in higher analysis, the particulars of which have appeared in full detail in our columns (vol. xxxii. p. 302); the prize works are to be published in the *Acta*.

The Methods of Glass-Blowing. By W. A. Shenstone. (Rivingtons, 1886.)

NOT only the student who is entering, or has just entered, that mystic land of chemical research, but also the ordinary student of chemistry who wishes to be more than a mere beginner, or a book-chemist, will hail with great joy the appearance of this little book on glass-blowing. We have not many good professional glass-blowers in this country, and, as the author says, it is a difficult if not impossible thing to get any instruction in glass-blowing; and, as a result, the great bulk of chemical students are as dependent on the dealers in glass ware as the bulk of amateur photographers are on dry plates and other things in that connection.

Most students even in our hardest working laboratories have some time to spare in which they might practise some of the more useful and simple methods of making glass apparatus mentioned and described in this little book.

Apart from the immediate utility of being able to make one's glass apparatus in the laboratory, and the help it is in almost any form of chemical or physical research, it cannot fail to be also indirectly useful to a student on his transplantation to a works or the superintendence of some technical operation, and will give him what is so very desirable, in addition to that of the purely chemical manipulation of analysis, a feeling of confidence in overcoming mechanical difficulties.

In the introduction is a description of a suitable working-place, blow-pipe, and bellows—things which are seldom to be found fit to use in our laboratories—and the blow-pipe flames; after which the varieties of glass mostly used, and the actual operations involved in the construction of most glass apparatus, in which glass tubes are the main parts, are very plainly described, being also in many cases illustrated with diagrams of the different stages. The last chapter is devoted to "calibrating and graduating glass apparatus," operations we think every student who gets as far as quantitative analysis should be able to perform in a decent manner.

We most thoroughly recommend this little book to students who intend to become chemists, and hope the proportion of those who can blow a respectable bulb will soon be increased. At present it is about ·1 per cent.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Two Hours with a "Subject Index"

THERE has appeared within the year, under the title of "A Subject-Index to the Modern Works added to the

Library of the British Museum in the Years 1880-85," the latest of the series of indexes published under the auspices of our great national library. Having recently had occasion to consult the same with reference to certain biological works, I experienced so much difficulty in finding what I wanted, that I thought it worth while to inquire more fully into the trustworthiness of the volume as an aid to the working naturalist. I append my notes, in the interest of workers in a field the literature of which is already overburdened.

The book is printed in unnecessarily large type, whence there results a ponderous volume of 1044 pages quarto. Upon the immensity of the task before the bibliographer set to compile such a work I need not comment. If the duty be well performed, there can only result a product useful to all, and helpful to the specialist; if otherwise, confusion becomes confounded, and there ensues to the worker loss of time, if not actual disaster.

Printers' errors and minor inaccuracies are to be expected in a work of the kind, and any such compilation must of necessity be faulty. It is not surprising, therefore, to find "travel" for "translation" in recording the title of Scott Russell's work, on p. 1016. Other similar defects could be quoted, but why are the press-marks occasionally omitted? surely they are next in importance to the titles themselves? The compositor cannot be at fault here.

The compiler sets forth his scheme in a short preface, by no means a bad one if followed. One of the chief provisions reads thus: "Headings and sub-headings are in strictly alphabetical order; precedence being given under each to the larger and more important works." It is in the execution of this self-imposed dictum that the compiler is seen at his best. Who is to be the judge of the relative merits of the collected works, and what is to be the standard of comparison? Presumably, the compiler performs the difficult task in person, but it is indeed well that his method is not revealed, for Huxley's "Crayfish" heads the list of general works upon "Mollusca" (p. 653), and Miss Buckley's "Life and her Children" that upon "Micro-organisms." The thing must be a joke! Nothing of the kind!! "Corallinen Algen" brings up the rear of "Corals" (p. 212), while "Les Batrachospermes" are exalted to an equality with the "Batrachians"—a truly novel means this of demonstrating the unity of the biological sciences. The aforenamed inaccuracies amount almost to incredibilities, and it is difficult to realise that they are perpetrated in earnest. It is inconceivable that they can have resulted from mere carelessness; but, even were that so, the fact that they exist at all would be sufficient in itself to disqualify the compiler. We can only regard them as the natural outcome of giving to one man the work of half a dozen; they disfigure the work, and shake our confidence in much that remains. A little less pardonable, perhaps, is the relegation of "Der Tasman-Gletscher" to the heading "Tasmania."

The choice of headings is often ill-advised. Four works are recorded upon the Polyzoa; these are equally divided between Polyzoa and Bryozoa, to neither of which headings is there a cross-reference. Writings upon organisms in air generally are distributed between sections Air and Atmosphere, Micro-organisms, and Bacilli and Bacteria, with no cross-references. Papers dealing with the broadest generalisations and the subtlest of details are muddled together regardless of system; while under Micro-organisms there is a cross-reference to Infusoria, sole possessor of which department is Kent's well-known Manual. Section Palæontology is especially introspective. It is divided into the sub-heads General, Bibliographical, Collections of Fossils &c., Fossil Fauna, and Flora—a sufficiently inconsistent arrangement in itself. Conspicuous among the general works is Brongniart's "Recherches sur les graines fossiles silicifiées." A knowledge of the publications of his own department ought certainly to be expected of the compiler, through whose hands the covers of the said works must surely pass. But no; for while there has been recorded under the last heading Rupert Jones's "Catalogue of Fossil Foraminifera," we look in vain for the companion volumes of Hinde and Lydekker, both of which were published within the period embraced. There is here something akin to contempt for authority! Marsh's "Odontornithes," divorced from Palæontology, finds a home among the Birds.

Having found that the leading tenet of the scheme was violated—that relating to the precedence of important works—I turned next to the second one. It reads: "All books are, so far as possible, grouped together according to their language, in the following order—English, French, German, Italian. . . . There are, however, some deviations from this system—for example, in the history of each country the titles follow each other in chronological sequence, irrespective of language." How the two methods of arrangement so vaguely formulated are to be simultaneously adopted, we are not informed; and in this ingenious attempt to combine two schemes into one neither has been adhered to—both have suffered. A cursory glance at some of the more important sections, *e.g.* Birds, shows that the conditions have not been in the least fulfilled, so far as pertains to precedence of language; while, to go afield, we find under Clergy, Priesthood, &c., a unique arrangement in which the languages run thus—Spanish, English, German, French, Italian, English, *ad finem*. Where, here, is the professed patriotism? It deserts the author under the inspiration of Clergy, Priesthood, &c., and leaves him reckless. What now of the chronological sequence? One turns instinctively to various historic headings. In none of those examined did I find the exception to be the rule.

The major scheme nullified, the preface becomes a blank; but the details of the superstructure are no more to be relied upon than is the foundation itself. Under Insects, to which the reader is referred from Entomology, there are titles in abundance in which the two catchwords occur; but he looks in vain for references to the fact that the majority of entomological papers are scattered among Coleoptera, Diptera, Hymenoptera, Lepidoptera, Ants, Wasps, Bees and Bee-keeping, and other headings. The Colorado Beetle figures under that heading and under Potato, but not under Coleoptera; while, to complicate matters, a German and an English work upon the subject appear under Potato, the place of the former being, under Colorado Beetle, usurped by a Spanish rival. Fish and Fisheries embraces all branches down to Oyster-Culture, but why not Salmon Fisheries? It appears to suffice that they should take shelter under Salmon, with no cross-references. A special heading is set up for Angling, but several definite works thereon are to be found only under Fish and Fisheries, no references being given under Angling.

Titles of works are not unfrequently repeated, and that unnecessarily. For example: Morris's "Letters about Birds" occurs under Birds (sub-headings—General, and Great Britain and Ireland); Dixon's, Swaysland's, and Watkins's books are not under their appropriate headings; while Patterson's "Birds, Fishes, and Cetacea frequenting Belfast Lough," which should be under Fauna, Local, is misplaced. Under the last-named heading Canon Tristram's "Fauna of Palestine" appears beneath sub-section Italy—the compiler seems doomed to disaster in his relations with the clergy. In one or two cases where comprehensive titles occur, a process of dismemberment has crept in. For example: Romanes' "Jelly-Fish, Star-Fish, and Sea-Urchins" is to be found under Star-Fish and Sea-Urchins, with the title mutilated beyond recognition, but not at all under either Jelly-Fish, Medusa, or Echinoderm; Scott Russell's "Wave of Translation in the Oceans of Water, Air, and Ether," to which allusion has already been made, occurs, also with a misquoted title, under Water, but under neither Ether nor Air and Atmosphere.

Such are the results of an attempt to gauge the working capacity of the "Index" mainly from a single stand-point. I leave other specialists to inquire for themselves. It has been said that this "work proves the ability and industry of its compiler, and that the Trustees are anxious to make the treasures of the great library a success." Granted the good intentions of the governing body and the industry of the compiler; of the rest the reader must judge for himself—I refrain from further comment thereon. It cannot, however, be admitted that the compiler has done his best; errors cannot be avoided where non-technical hands are applied to technical work, but slovenliness is ever intolerable. I submit that a distribution of labour is demanded for the success of the next essay of the kind.

JUSTITIA

The Origin of Species

IN a recent issue of NATURE (Nov. 25, p. 77) Mr. Catchpool writes:—"Is it, or is it not, the fact that allied species, which are confined each to a particular island, prove, when brought together, far less frequently infertile than species, equally dissimilar, which had lived in the same district, might be expected to prove? On the answer to this question depends, as far as I can see, the fate of the theory of physiological selection. Can no one answer it?"

This is a question which has arisen out of the theory of physiological selection itself, and, as Mr. Catchpool was the first to propound that theory, I am afraid he will find that no one as yet is able to answer his question. But it may interest some of your readers to know that I have collected a considerable body of facts tending to show that there is a correlation between fertility of allied species and the fact of their living on isolated areas, as well as another correlation between sterility of allied species and the fact of their living on continuous areas. But it seemed to me undesirable to publish these facts until very largely augmented; and, finding that no observations had been expressly made upon the subject, I read my paper before the Linnean Society for the purpose of inducing naturalists in different parts of the world to try experiments on the fertility of nearly allied species inhabiting isolated areas, as well as to test for degrees of sterility between natural varieties and parent forms on continuous areas. Meanwhile I am myself trying experiments on the mutual fertility of isolated species, as well as upon the sterility of natural varieties with parent forms. Where birds and mammals are concerned, I am using the numberless islands of all sizes on the west coast of Scotland, which are admirably suited to the purpose. Any of your readers in any parts of the world who are acquainted with well-marked natural varieties of birds or mammals which (together with their parent forms) would be likely to thrive on these islands, would greatly oblige me by communicating suggestions.

I may take this opportunity of also requesting any of your readers who may have further remarks or criticisms to make on the theory of physiological selection not to delay their publication. For it is surely desirable that discussion of the subject "on the high *priori* road" should come to an end. Until a large number of experiments shall have been made, any definite judgment upon the theory must be either biassed or premature; and therefore the only influence that can now be exercised by adverse criticism is that of discouraging the work of verification. On this account, and on this account alone, is it worth one's while to answer such criticism. Hitherto I have waited till it should come to an end, and withdrawn the answer previously referred to in these columns as having been in type for the *Fortnightly Review*. When it has come to an end, I will furnish a general reply in the *Nineteenth Century*, and shall then hope to show that whatever "fate" may be in store for the theory at the hands of Nature, at all events it is certain that it has been in no way affected by the assault of naturalists.

George J. Romanes

Heredity in Abnormal-Toed Cats

The calculation made by Mr. J. H. Hodd in last week's Nature (p. 53) of the numerical proportion of the sexes of Mr. Poulton's race of extra-toed cats, in relation to the recurrence of the abnormity, is very interesting as an inferrable deduction from the premises of Mr. Poulton's elaborate tabulation of his observations; but it is, I think, doubtful whether it is not misleading as a generalisation from the facts collected by him. A conclusion arrived at by means of a mathematical method is too absolute an inference, and not necessarily reliable, when applied to purely biological cases, on account of the numerous intervening factors (perhaps mostly unknown, but yet of importance) which are incapable of being appreciated within such an estimate, and which may, in consequence, invalidate the main point under consideration. This appears to be the case on the present occasion; for Mr. Poulton mentions, in describing the kittens of VIII^5 (p. 39), that by far the most highly specialised development of the abnormity he has observed, throughout the entire race, was attained in a *male*—VIII^5 iv. Considering that the large majority of individuals possessing the character are of the ♀ sex, as pointed out by Mr. Hodd, this fact is really very striking. But, at the same time, if the point alluded to by Mr. Hodd were considered as amounting to a principle, we should naturally be led to infer that the abnormity might become equivalent to a secondary sexual character, which is so improbable that it scarcely bears suggestion. Besides, it is necessary to bear in mind that all the observations carried on as yet by Mr. Poulton have dealt *with the ♀ influence alone;* and it may be accounted for by the very reason that all the ♂ s have been carefully eliminated from exercising any influence on the race, that the predominant effect, *numerically*, is on the ♀ side; while it is most probable that if an abnormal ♂ were selected (say, the individual just referred to) as a starting-point for experiment with normal ♀ s, the result would exhibit a general tendency towards superior persistence of the character operating with greater potency amongst the males than the females. It would be very interesting to experiment in this manner under the isolation happily promised in the Madeiran Islands.

Mr. Hodd's statistical statement is, doubtless, correct in the main, under the restricted provision just suggested, only one must not attach too great importance to it. The preponderance in number of the ♀ s over ♂ s is, indeed, still more noticeable when the ratio of each sex to the total number of the thirty-six cats is considered. Thus it appears that of twelve ♂ s 13$\frac{8}{9}$ per cent. are normal, and only 19$\frac{4}{9}$ per cent. abnormal; whilst of the twenty-four ♀ s 19$\frac{4}{9}$ per cent. are normal, as many as 47$\frac{8}{9}$ per cent. are abnormal. It is perhaps a little premature to place entire reliance upon so small a number, but it will prove interesting to compare the statistics brought out in Mr. Hodd's letter with a larger number in time to come.

In considering such a close and carefully detailed analysis as that presented by Mr. Poulton, it is of no little interest to find, on referring to that wealth of facts and principles, "The Descent of Man," that these results are in complete accord with, and confirm the laws of inheritance formulated by Darwin, if the strictures imposed by him, as if in anticipation of future observations, are duly regarded; and as his remarks apply to the present subject very directly,—although bearing more especially upon sexual selection in general,—I cannot do better than quote some of the more pithy:—"Why certain characters should be inherited by both sexes, and other characters by one sex alone, namely by that sex in which the character first appeared, is in most cases quite unknown. We cannot even conjecture why with certain sub-breeds of the pigeon, black striæ, though transmitted through the female, should be developed in the male alone, whilst every other character is equally transferred to both sexes; why, again, with cats, the tortoiseshell colour should, with rare exceptions, be developed in the female alone" ("Descent," 2nd ed., p. 232). It is curious that the cats under observation happen to be tabby-tortoiseshell, and the remark made by Darwin (p. 230), that "as a rule it is the females alone in cats which are tortoiseshell, the corresponding colour in the males being rusty red," fully obtains in the present case, since the ♂ s are, unless tabby, sandy-coloured, but never tortoiseshell. It is furthermore important to notice that the sandy individuals have the supernumerary digits as fully developed as any of the ♀ s in the same litter.

Darwin's explanation of the persistence of abnormal characters is on the lines of the theory of pangenesis, as follows:—

"It is in itself probable that any character appearing at an early age would tend to be inherited equally by both sexes, for the sexes do not differ much in constitution before the power of reproduction is gained. On the other hand, after this power has been gained, and the sexes have come to differ in constitution, the gemmules (if I may again use the language of pangenesis), which are cast off from each varying part of the one sex, would be much more likely to possess the proper affinities for uniting with the tissues of the same sex, and thus becoming developed, than in the other sex" (p. 232). On p. 237 we find the further remark:—"The presence of supernumerary digits, and the absence of certain phalanges, must be determined at an early embryonic period, ... yet these peculiarities, and other similar ones, are often limited in their transmission to one sex: so that the rule that characters developed at an early period tend to be transmitted to both sexes here wholly fails."[1]

And, in conclusion:—"Characters of the parents often, or even generally, tend to become developed in the offspring of the same sex, at the same age, ... in which they first appeared in the parents. *But these rules, owing to unknown causes, are far from being fixed.* Hence, during the modification of a species, the successive changes may readily be transmitted in different ways; some to one sex, and some to both; some to the offspring at one age, and some to the offspring at all ages. Not only are the laws of inheritance extremely complex, but so are the causes which induce and govern variability" (p. 240).

It is quite remarkable, though it is not at all surprising, how closely Mr. Poulton's facts fit into these deductions of Darwin's, drawn, as they were, from such few instances that they seem to be little less than preconceived ideas.

But, as it may be due to a one-sided influence in special cases that an effect is found to have clung with so much persistence to one sex, I am inclined to believe that, upon the experiment being made as I have suggested, that the other sex will produce similar results in regard to the numerical proportion of the sexes, and a strong point in favour of this opinion lies in the fact that, as I have myself seen, a sandy-coloured ♂ kitten, apparently bearing the stamp of the normal ♂ parent, was found to bear the development of the supernumerary digits in a marked degree. WILLIAM WHITE

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[1] This is a point which perhaps requires further evidence.

55, Highbury Hill, N., November 22

Algebraic Notation of Kinship

WITH reference to Mr. Davison's letter in NATURE (vol. xxxiv. p. 571), I wish to point out that the subject of algebraic notation, not only for kinship, but for kinship and affinity, has been pretty fully discussed in several papers which I contributed to the Royal Society of Edinburgh, and especially in a paper entitled "Analysis of Relationships of Consanguinity and Affinity," which, at the request of Mr. Galton and Dr. Tylor, I contributed to the Anthropological Institute (*Journal* of the Anthropological Institute, August 1882). Some idea of the nature of that paper may be got from a statement of the several tables which are appended to it. Table I. gives the notation for the general relations of the first five orders, states the general and singular meaning of each, and classifies them according to index, sign, and grade. Table II. shows how these general relationships are divided into ultimate species. Table III. gives all the possible relationships of a man to a woman, and of a woman to a man, within the first five orders; and such relationships as exclude marriage according to the laws of England are marked with an asterisk. Table IV. gives the consanguineous relationships of the first five orders grouped in lines and species, the agnatic system being formed by the extreme terms on the left, and the uterine system by the extreme terms on the right. Table V. gives strict definitions of the English terms of relationship.

Besides the algebraic notation, I also developed a graphical notation. In the paper referred to, I apply the graphical notation to show the descent of property according to the English law.

Prof. Jevons took much interest in these papers, and it was his intention to give the elements of the analysis in a new chapter of his "Studies in Deductive Logic," but death snatched him from us in the midst of his scientific labours.

ALEXANDER MACFARLANE

University of Texas, Rustin, Texas, November 15

Seismometry

IN the last number of NATURE (p. 75) there appears a letter by Prof. J. A. Ewing, referring to a note in a previous number (p. 36), apparently a summary of a communication from Prof. Milne. As I have some interest in this question, and have reason to believe from remarks made in a letter lately received from Prof. Milne that the matters referred to by Prof. Ewing cannot be those to which Prof. Milne referred, I should be glad if the original communication could be published.

Prof. Ewing's letter and indeed several of his recent publications, including the description of his instruments in NATURE, are decidedly calculated to mislead those not familiar with the seismological work which has been done in Japan. For example, he says, or leads one to infer, that he introduced horizontal pendulums in seismology: now that is not the case. It is needless for me to say that horizontal pendulums have long been known as a means of obtaining nearly neutral equilibrium; and in particular, with reference to Japan, they are referred to by Prof. Milne on page 25 of vol. i. part 1 of the *Trans. Seis. Soc. Japan*, in a paper which was read in Prof. Ewing's presence several months before Prof. Ewing's instrument was heard of. What Prof. Ewing did introduce was a particular form of horizontal pendulum, very particularly described by him in some of his early papers, as involving a "new principle" (now apparently abandoned by him), and he used two such pendulums to write two components of the earth's motion on a *continuously* moving plate. Records on moving surfaces were not new then, even in Japan, as they are referred to in papers published by other investigators before Prof. Ewing arrived in the country, but there was this difference in these older methods, that the plates were automatically started by the earthquake; and Prof. Ewing, after his experience, has now adopted this plan.

Prof. Ewing mentions also in his letter that his apparatus writes three components of the motion, but he does not say that the most difficult of the three—namely, the vertical component—is written by an instrument which I introduced and described before the Seismological Society of Japan, first in May 1880, and afterwards in a modified form in April 1881. Prof. Ewing's instrument is professedly, as his first description (*Trans. Seis. Soc. Japan*, vol. iii.) clearly shows, a modification of my second form, and is, what he seems persistently to have shut his eyes to, almost identical with my early form.

As to Prof. Ewing's statement in the last sentence of his letter that "there is nothing better to take their place," we can hardly be expected to take *his* judgment on this point.

THOMAS GRAY

7, Broomhill Avenue, Partick, Glasgow, November 30

[Nothing essential was omitted from Prof. Milne's letter.—ED.]

Botanical Lecture Experiment

THE following simple lecture-experiment may interest teachers of botany. It is described by Georg Klebs in his paper "Ueber d. Organisation d. Gallerte bei einigen Algen u. Flagellaten," published in the most recent part of *Unters. a. d. bot. Inst. z. Tubingen*. A description of the experiment I give in Klebs's words, translated:—"It is easy to demonstrate, by addition of a watery solution of phenolphtalein, that Algæ make the water in which they live alkaline when they are fixing carbon in light. In proportion as the fixation of carbon proceeds, the water gradually assumes a deep red tinge, which gradually disappears when light is excluded." The explanation given is:—"The Alga takes up not only the CO_2 absorbed in the water, but also in part that which is in combination in acid carbonates, in consequence of which alkaline combinations arise; in darkness, owing to respiration, the reverse process takes place."

I have a vessel with water containing phenolphtalein in which Cladophora has grown for nearly three weeks, and there is daily a reddening of the water, its rapidity being determined by the brightness of the day; during the night the colour disappears.

BAYLEY BALFOUR

A Lecture Experiment on the Expansion of Solids by Heat

MR. MADAN'S description of a device for showing that metals and solids expand when exposed to heat is very interesting, especially as such an arrangement, but with important modifications, is capable of giving very excellent scientific results, results which are only surpassed by M. Fizeau's method. One necessary alteration is the substitution of a spring-pressure for the weight on the strip of metal. This and other points will be made quite clear by a perusal of a short description contained in my paper, "A Strain-Indicator for Use at Sea," read before the Institution of Naval Architects. The numerous tables and diagrams there given would, I am afraid, hardly interest your readers, but the repeated experiments in Table I. would be a subject of interest, as they show how well the experiments agree amongst themselves. The errors, though small, are due, in my opinion, not only to the difficulty of reading the dial (each unit being equal to about half an inch, and the second decimal therefore about 1/200 inch), but also to the difficulty of reading the exact position of the weight on the steel-yard of the testing-machine. Far more accurate results are obtained when, instead of a jockey weight being run out, small weights are added one by one.

You will also notice that the instrument gives very good records on paper (see launching strain diagrams and railway bridge diagrams), and in this shape it could, I think, be used with advantage for recording changes of temperature.

C. E. STROMEYER

Strawberry Hill, Middlesex, December 2

Meteors and Auroras

IN the *Proceedings* of the Paris Academy of Sciences published in NATURE for November 4, at p. 23, a relation between showers of shooting-stars and auroras is noted. In this vicinity on April 14, May 8, July 27, and November 2, very fine auroras were visible, and upon each occasion shooting-stars of unusual brilliancy were observed in the northern heavens whilst the aurora was at its height. M. A. VEEDER

Lyons, New York, November 24

THE GUTHRIE MEMORIAL FUND

A COMMITTEE has been formed, under the presidency of Prof. Huxley, to raise a memorial fund in honour of the late Prof. Guthrie, F.R.S. Prof. Guthrie endeared himself to a large circle of friends by his simple character and wide sympathies. Unfortunately, as his time was exclusively devoted to teaching and to scientific research, the provision for his family is far from adequate. A slender income is furnished for his widow by a policy of insurance settled upon herself, but this will not enable her to provide for the education and maintenance of her step-children. The ages of the children dependent upon her are twelve, fourteen, and seventeen years respectively, and their case is the more sad because, until a late period of his life, Dr. Guthrie had every reason to be satisfied that they were sufficiently provided for.

Under these circumstances it will be felt by all who value his memory, as well as by those who only knew him through his scientific labours, that any sum which is gathered as a memorial of his life must necessarily be devoted to placing his children as nearly as may be possible in the position they would have occupied but for his untimely death.

Subscriptions may be sent to the Honorary Treasurer, Major Macgregor, R.E., Science Schools, South Kensington Museum, London, S.W.; or to the Honorary Secretary of the fund, Mr. C. Vernon Boys, at the same address. Cheques to be crossed "Messrs. Cox and Co."

In addition to the gentlemen named above, the Executive Committee consists of Capt. Abney, Prof. W. G. Adams, Prof. Roberts-Austen, Walter Besant, Prof. G. Carey Foster, Dr. J. H. Gladstone, W. J. Harrison, J. Power Hicks, Prof. J. W. Judd, Prof. A. W. Reinold, and Prof. Balfour Stewart; besides whom there is a General Committee, comprising Prof. W. E. Ayrton, Shelford Bidwell, Walter Bailey, T. Lauder Brunton, W. H. M. Christie, Prof. Clifton, Conrad Cooke, Prof. Crookes, Warren De La Rue, Prof. Dewar, Colonel Donnelly, General Festing, Prof. G. Forbes, Prof. Fuller, R. T. Glazebrook, Prof. Goodeve, Dr. Hopkinson, J. Norman Lockyer, Sir John Lubbock, Bart., Prof. MacLeod, Prof. J. Perry, Prof. Poynting, Prof. Rücker, Dr. W. J. Russell, Prof. W. A. Tilden, Prof. S. P. Thompson, Prof. Thorpe, and Dr. Alder Wright.

It is satisfactory to hear that already a considerable number of subscriptions have been received, but it is hoped that when the necessity for the existence of such a fund shall become better known there may be a large increase in the number.

VOLCANIC ERUPTION IN NIUA-FU, FRIENDLY ISLANDS

SIR J. H. LEFROY has forwarded to me a small packet of volcanic dust, together with an extract from a letter written by Mr. Coutts Trotter, F.R.G.S., and has requested me to examine the former and append my remarks upon it to the more important parts of Mr. Trotter's letter. This document is dated on September 24, 1886, "on board the ss. *Suva*, a few miles south of the Island of Niua-foou" (or Niua-fu, one of the Friendly Islands). After speaking of an expedition to Fiji, Mr. Trotter proceeds:—

"Meanwhile I got into a little steamer to visit the windward island of the group, and was persuaded to come on in her to Tonga. There I found that news had just come of an awful volcanic eruption in the Island of Niua-foou above mentioned, and my steamer was chartered to go and make inquiries and give relief. . . . We started at once, and arrived off the island before dark yesterday. No trace of fire or smoke, and I was much chaffed for my 'disappointment.' But on landing this morning we found the damage done was substantial enough, an eruption of dust and stones and water having gone on for eighteen days, and two-thirds of the island smothered or greatly injured. The island is some forty or fifty miles round, all volcanic, no beach anywhere, and landing difficult, and a lake of brackish-bitter water occupying perhaps a fourth or more of its extent. There are at all events three small islands in the lake, one with a lake in its centre. I suspect this lake is the remains of the crater and eruption to which the existence of the island is due, later eruptions being cause for the small island craters. The present eruption began apparently near one end of the lake. I saw three or four craters there—one covered with a green sulphurous scum; and another, just beyond it, which I could not in the time I had actually visit, very deep, and full (a friend tells me) of mud and water. Near it is a little rounded mountain of 'earth,' some 200 feet high, formed by the present eruption, and projecting far into the lake; at the other end of the lake is a fresh accumulation, as I was told, of pumice, but it looked to me from where I stood more like an accumulation of black sand. The whole island has been in a disturbed state for some three months and a half, the dates of the principal disturbances coinciding remarkably with those which are going on in other parts of the world—earthquakes on June 8 and 11, which I think are the dates of the first New Zealand outbreaks,[1] again on August 12, ditto. This of course is not wonderful, but the final catastrophe here took place on August 31, which we understand was the exact date of the recent American earthquake.[2] It was preceded for twenty-four hours by earthquakes, . . . and went on for ten days, I am told, without intermission, then two days quiet interval, then going on again for nearly a week—terrific thunder and lightning for twenty-four hours incessantly. The column of steam rose, they say, several thousand feet, anyhow immensely higher than a hill 7600 feet high, which I ascended, and whence I had a bird's-eye view of the lake and crater. Showers of stone accompanied it; these fortunately fell straight, or nearly straight, back. They were red-hot, with masses of dust attached, and as they fell left the dust behind, which produced the effect of a fiery tail. The great mischief was done by the dust, which, as the wind shifted, carried destruction in every direction. In one village which I entered, the shower only lasted an hour and a half, but the ground was deeply covered, the blades of grass even now only beginning to peep through, and every coco-palm ruined for the present, the branches hanging withered and almost perpendicular, and the young central shoot sticking out by itself. If they get rain, the trees will recover and bear again in three years, but otherwise are likely to die. But in other districts the houses are buried, and along the coast large extents of forest, scrub, or bush, and, what is more immediately serious, the yam beds. They have just been planted, and any that were above ground will be killed, even if the latest planted may push through and flourish. Wonderful to say, no one was killed, although many very old people have died since from fear and exhaustion. They all betook themselves to the upper parts of the island for safety, and perhaps with reason, for the last two volcanic outbursts both took place on the coast-country near the shore. These (respectively nineteen and forty years ago) were both lava eruptions. I saw the craters and the lava-streams from them down to the sea on the west coast as we steamed along to-day; the lava of the earliest being hardly invaded yet by vegetation —not a blade of green on the later, which runs far out into the sea, like the rough substratum for a big embankment or breakwater. According to native tradition, the last eruption of a kind similar to the present took place from very nearly the same spot in the lake seventy-two years ago, the old people having childish recollections yet. The lake is a great depth, so that this hill of 200 feet or more rising from the bottom represents a vast amount of solid matter, to say nothing of the thick deposits of dust all over the island. The lake was still bubbling in places, and things are by no means settled down yet. At Vavau, where we touched two days ago, they had just had a very severe earthquake, and shocks are still going on at Niuafoou (vertical, *I was told*, but my informant's wits were much shaken by recent events) daily on the level ground near our landing-place, from which it is inferred that the danger is not over. Strong gases too are perceptible rising from the ground near the coast, which is always where they apprehend most danger, and an outburst of lava. I suppose the solid matter coming up through the deep lake is pulverised into the (to life) comparatively harmless dust. During the earthquake of August 12, the captain of a ship at anchor found that, whereas he had paid out twenty fathoms of chain over-night, he had only eight fathoms under him in the morning. I never saw such big coco-nuts anywhere, though the trees are not exceptionally big, indeed there seem to be no very fine or old trees of any kind on the island, which favours the theory of a modern origin, for the soil is very fertile. The name means New Niua, the Old Niua being probably the neighbouring Keppel Island or Niua-tobu-dabu. I wish I could give you a better or fuller and more interesting account of the whole affair, but the visit was a very hurried one, and, in fact, I had not more than two hours on shore. Still it may interest you, as it is written on the very spot: no other account is likely to reach England. I send a pinch of 'sand' from the crater. "C. T."

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[1] The first outbreak was early on the morning of June 10. See NATURE, vol. xxxiv. p. 301.

[2] The principal shock was on Tuesday night, August 31. See NATURE, vol. xxxiv. p. 470, and vol. xxxv. p. 31.

This "sand" or "dust" is a very dark-brown—almost black—colour. When examined with a lens it seems composed mainly of fragments of glass, and has a slightly speckled aspect, owing to the mixture of lighter and darker fragments; one or two glassy-white fragments may also be noted. When some of the dust is placed under the microscope, it is seen to consist almost wholly of fragments—some rudely polygonal in shape, others flattish chips—of a brown glass; the former being the commoner. The majority of the bits vary from about ·01 inch to ·03 inch in diameter, and the latter measurement is but rarely exceeded. Minute chips are also present, but they do not form at all an important constituent in the mass. A conspicuous characteristic is the (apparently) entire absence of the tiny pellets of "cindery" scoria, so frequent a constituent of volcanic dust, and of the fine pulverulent material, the presence of which commonly makes it needful to mount the dust on a slide before it can be properly studied. I have found no difficulty in examining this Niua-fu dust, and even the finer chips—often less than ·001 inch in diameter—by simply spreading it over a sheet of glass. The glass fragments, even when very minute, have a tinge of brown: when about ·01 inch in thickness, they are fairly translucent, and a rich olive-brown in colour; but as they approach ·03 inch in thickness they become opaque, light only passing through the thinner edges. Small cavities, spherical, or egg-shaped, are not infrequent, but the glass is remarkably free from microlithic inclosures. No granulation of the colouring-matter is perceptible, as a rule, with a magnification of 150 diameters; opacite dust and trichites (especially the latter) are very rare; and of other microlithic inclosures I have only seen an occasional lath-shaped crystallite (? feldspar). I have not identified among the fragments either biotite, augite, or hornblende; so that if any of these minerals are present they must be very rare. The clear glassy fragments mentioned above are feldspar—probably labradorite. They do not in number exceed about 2 or 3 per cent. of the whole. Many of the flatter brown-glass fragments exhibit ropy folds or the remains of a cellular structure, evidencing that they are due to the destruction of a very vesicular glass, while the more solid polygonal fragments may be the detritus of the thicker parts of the same or of a more uniform glass. The strong brown colour of the fragments reminds me of specimens of the more glassy lavas of the Sandwich Islands in my collection; and like them I should, from microscopic examination, consider the rock a basalt-glass (tachylyte) with a silica percentage, which was probably above rather than below 50. This view accords, I find, with Cohen's statement concerning the lava of Niua-fu, which, judging from his description, is very similar to that above described (*Neues Jahrb. für Min.* 1880, vol. ii. pp. 36 and 41); he says that it is almost identical in composition with the "basalt-obsidians" (*i.e.* tachylytes) of the Sandwich Islands. It contains 50·74 of silica; their analyses show from 50·82 to 53·81.

While the above was passing through the press, I received from my friend Dr. S. Rideal a determination of the specific gravity of the volcanic material (powdered to get rid of cavities). The specific gravity is 2·726. As the feldspar is included, and it is slightly the lighter, the specific gravity of the glass itself must be a little higher, about 2·73. Hence we need not hesitate to call it a tachylyte. The average of six Sandwich Island glasses is 2·71 (see Judd, *Q. J. G. S.*, xxxix. 444).

T. G. BONNEY

FOURTH ANNUAL REPORT OF THE FISHERY BOARD FOR SCOTLAND

THE Report of the Fishery Board for Scotland increases each year, not only in size, but in interest for the general public, as well as for those readers whom it specially concerns; and, unlike ordinary Blue-books, its pages are to a large extent devoted to scientific papers which appeal to many not directly concerned with the fishery industry.

The herring-fisheries continue to be most productive. A very striking feature of the summer herring-fishery of 1885 is, that many in-shore grounds, where herrings were previously found in great abundance, but which had recently been all but deserted, were restored to their former fertility. The increase of the herring-fishery in the Shetland district, which now ranks as the most important in the country, still continues, greatly to the improvement of the condition both of the people and of their boats. The fish are of finer quality than those taken on other parts of the east coast. The takes of other sea fish and salmon were also very large. The gross total estimated value of the sea and salmon-fisheries for Scotland was 2,859,822*l.* 15*s.* The Board have already expressed their regret that so many tons of sprats are annually used as manure. Could they be transmitted to populous districts at a reasonable rate, they would be a cheap and valuable addition to the food-supply, or, where this was impracticable, preserved as anchovies as in Norway, or as sardines as in Canada. The importance of utilising the by-products of the fisheries is now widely recognised. Papers by Dr. Stirling and Mr. Haliburton give an account of certain economical products obtained from fish, and experiments are being made on a fairly large scale by Mr. Sahlström at Aberdeen, which may, it is hoped, lead to some practical results. Investigations on whitebait by Prof. Ewart and Mr. Matthews showed it to consist almost entirely, and at all seasons, of young sprats and young herrings, varying according to the season of the year and the place of capture. It might, therefore, be advantageous for the Firth of Forth and other in-shore waters to send supplies of whitebait to the English markets.

The Scientific Committee of the Board had the assistance of Mr. Brook, Prof. Stirling, and Mr. Duncan Matthews, of Aberdeen; Prof. McIntosh, of St. Andrews; Prof. Greenfield and Dr. Gibson, University of Edinburgh; the Rev. A. M. Norman, D.C.L. of Durham; and Mr. Haliburton, of University College, London. Though the Marine Laboratories at Tarbert and St. Andrews admitted of several important inquiries being initiated, the Board is still greatly hampered for want of proper boats, and throughout the year the limited amount of dredging and field-work done was rendered possible by boats supplied by Prof. Ewart. It is to be hoped that arrangements will be made by the Admiralty, which will admit of the superintending vessels undertaking a complete survey of the spawning-banks and assisting in work of a like nature when required. A small steamer has already been provided for trawling experiments and other work on the east coast. At the Rothesay Aquarium the spawning of the cod was studied by Prof. Ewart and Mr. Brook, who generally confirmed the observations of Sars, and gained further information as to the natural and artificial fertilisation of the eggs, and their buoyancy before and after fertilisation in different kinds of water. The appendix contains part ii. of a paper by Mr. Brook on the development of the herring. Experiments made in artificial fertilisation of herring eggs appear to justify the following conclusions:—

(1) The ova retain their vitality, and are capable of being fertilised from forty to forty-eight hours after the female is dead. In the experiments performed, forty-eight hours seems to be a little outside the limit at which the eggs are capable of being fertilised, but it is probable that temperature may have an influence on the vitality of the ova. (2) The spermatozoa do not retain their vitality for nearly so long a period. Three hours appears to be the limit indicated by the above experiments. (3) The egg capsule never separates from the yolk excepting under the influence of spermatozoa. It would appear that when the ova and spermatozoa have partly lost their vitality a partial separation of the egg membrane from the yolk may take place, although the ovum is not truly fertilised. Experiments proved conclusively that the egg capsule is not permeable to water until after it has been penetrated by spermatozoa. (4) The egg membrane is covered with a viscous secretion when the ovum leaves the oviduct, which serves for the attachment of the ovum. This viscous layer gradually hardens in sea-water. Active spermatozoa are able to penetrate this layer some hours after it has set, but this power appears to be confined to the first twenty-four hours after deposition. (5) There is no collection of germinal protoplasm at the surface of the yolk in the ripe ovarian ovum, nor is a germinal disk ever found so long as an ovum remains unfertilised. The formation of the germinal disk cannot be made out in living ova, and its true nature can only be determined from a study of sections. Investigations on the formation of the blastoderm, and examination of a large number of sections, lead to the conclusion that the animal pole of the ovum gives rise to the ectoderm. In many forms the animal pole at the time of the formation of the segmentation cavity consists only of true archeblast cells. In the herring, and probably some other forms, the animal pole receives an addition of cells from the parablast prior to the formation of the segmentation cavity. The primitive hypoblast, which is almost entirely derived from the parablast (*i.e.* from the vegetative pole), gives rise to the mesoderm, and the secondary hypoblast (endoderm) remains as a single row of cells in connection with the parablast.

If these conclusions are correct, the similarity between the development of teleosteans and amphibians (*Rana*) cannot fail to be noted. The derivatives of the animal and vegetative poles are in both cases practically identical. The secondary segmentation (budding) in the parablast of teleosteans must then be regarded as the necessary consequence of the relative distribution of protoplasm and yolk in the vegetative pole. The primitive hypoblast, as here described for the herring, is precisely homologous with that of Amphioxus. In both cases the primitive layer gives rise to mesoderm, notochord, and true endoderm. The position here brought forward is one advocated by Mr. Brook over a year ago, but from the nature of the material then at his disposal he failed to observe the details of the process. Quite recently, Dr. Ruckert, who has been studying the behaviour of the parablast in Elasmobranchs, has come to conclusions practically identical with those here advocated for Teleosteans.

The question, "Are herring ova likely to develop normally on the deep off-shore fishing-banks?" is discussed by Prof. Ewart in a way which shows that the Board aims at practical results as much as mere scientific investigations.

Until comparatively recent years nearly all the herring taken in summer were captured by small boats within a few miles from the shore. In 1852, *e.g.*, immense herring shoals reached the Moray Firth to spawn on the Guillam and other in-shore banks. Since 1852 the fishing boats have greatly increased in size, and owing to the introduction of cotton nets, each boat has added greatly to its catching power.

As the boats have increased in size and sea-worthiness, the fishermen have proceeded farther and farther to sea in search of the herring shoals, and now the greater number of the herring are taken from forty to sixty miles from the coast. It is often alleged that it was owing to the herring deserting the in-shore grounds that the fishermen proceeded to sea in search of the shoals, and also that it is because the fishermen disturb and break up the shoals early in the season that they no longer or seldom visit their old spawning-grounds. There is no doubt that during the last fifteen years comparatively few herring have been captured during the summer over the in-shore banks of the Moray Firth; but whether this is the result (as is alleged) of the fishermen intercepting and breaking up the shoals before they have had time to reach the in-shore ground it is impossible to say. It is, however, a question of great interest, and one which could in all probability be easily settled. If the fishermen were to refrain from fishing for one year in the various districts along the east coast until the fish had reached maturity, this problem would most likely be solved. In all probability the herring would be found as abundantly as in former years in the in-shore waters, and the fish captured would be larger and riper than those taken early in the fishing-season of former years from the corresponding shoals.

That the takes during recent years have consisted chiefly of small fish (so-called maties), will be evident by a reference to Reports of the Fishery Board. It is generally admitted that the great depression of the fishery industry which now prevails would, to a great extent, have been prevented if half of the small herring (the maties) had been left in the sea. Many of those who account for fewer herring being captured in-shore, by saying it is impossible for them to run the gauntlet of the thousands of nets that are night after night drifting across their path, assert that the eggs are incapable of developing in deep water, and that in course of time the off-shore shoals will diminish or disappear. Of this there is in the meantime no evidence. As a matter of fact, for all we know there may have been immense shoals of herring spawning on the banks which lie at a distance of from thirty to sixty miles off the Scottish coast for centuries.

The existence and continuance of off-shore shoals will, to a great extent, depend on whether the herring are able to reproduce themselves without visiting the in-shore spawning-banks, and the success of the herring industry will depend on whether the herring shoals, which are invaded annually by our fishing fleet, continue to spawn sufficiently near the coast to render their capture a profitable enterprise for our fishermen. For this it is necessary to have large shoals moving in limited areas, and these are only found on the east coast during the winter and summer spawning-seasons. If herring ova are capable of hatching in deep water (say from 60 to 100 fathoms) it may be taken for granted that any of the many gravel-coated banks of the North Sea may serve as spawning-beds. The North Sea is remarkably shallow, there being only one small area near our shores (generally known as the "Pot," and lying two to five miles off Fraserburgh), where a depth of 100 fathoms is reached. The most certain way of proving whether herring ova hatch or not in deep water would be to dredge herring spawn from one of the off-shore banks in an advanced stage of development; but, after several unsuccessful attempts to do this, it occurred to Prof. Ewart that the question might be practically settled by depositing fertilised eggs in specially constructed hatching-boxes in deep water. This was done in the "Pot," but without result, as a storm swept away all traces either of buoys or boxes. The Moray Firth being in many respects unsuitable for this experiment, Prof. Ewart turned his attention to the West Coast, and found a comparatively sheltered spot in Loch Fyne, with a depth of 104 fathoms. To insure success, a small tank was constructed of thick slate slabs firmly bound together by iron rods. The tank, though only about 20 inches square, weighed nearly 2 cwts. In the top and in two sides of this tank small windows were made about 6 inches square. Each window was carefully fitted with a teak frame, across which a single layer of horsehair cloth was stretched. These windows admitted a sufficient current of water to pass through the tank. All the necessary preparations having been made for depositing the tank during last autumn. Mr. Brook, who was engaged at the Fishery Board Tarbert Station during the autumn, undertook to obtain eggs and superintend the sinking of the tank in the 100-fathom water. Eggs were obtained on September 11 from herring caught in Kilbrannan Sound in water varying from 8 to 12 fathoms. All the eggs were placed at first in the laboratory in water which had an average temperature of 54° F. Most of the eggs kept in the laboratory hatched out on the 19th, while others only hatched on the 24th, thirteen days after fertilisation. On the 16th, one of the glass plates, coated with eggs, was introduced into the tank above mentioned, which was immediately conveyed to the middle of the channel, and deposited in 98 fathoms water, about three miles off Tarbert. The surface temperature was 54° F., the bottom temperature was 49° F. The bottom around the tank was chiefly composed of mud. On the 24th—*i.e.* thirteen days after fertilisation, and eight days after the eggs were deposited in 98 fathoms water—the tank was raised. On examining the glass plate, it was found a number of the eggs in the centre had been destroyed by a fine coating of mud, which had entered through the hair-cloth screen, while those near the margins contained vigorous embryos almost ready to hatch; in a few cases hatching had taken place. The average bottom temperature while the eggs were deposited was 49°·3 F.; the average surface temperature, 54° F.,—the difference being 4°·7. The difference of 4°·7 during the eight days which the eggs were deposited delayed hatching for about five days. This experiment clearly shows that the only difference between the hatching of herring ova in deep and shallow water is one of time; hence we are safe in concluding that if herring deposit their eggs on suitable ground, in any depth of water not exceeding 100 fathoms, they will undergo development. It is conceivable, however, that the depth of the water in which the eggs are deposited may have some influence on the time of spawning—in other words, on the fishing-season; and the immature condition of the fish caught in August during recent years may to some extent be accounted for in this way. If the herring which formerly spawned on the in-shore banks of the Moray Firth in from 10 to 20 fathoms water now spawn off-shore in from 40 to 60 fathoms water, the hatching will be delayed for several days, and maturity will not be reached as early as formerly. This is an argument in favour of beginning the herring-fishing later in the season than at present. It may be objected that the fry, if hatched out in deep water, would never succeed in reaching the surface; and supposing that they could, the necessary food might not be found forty to sixty miles from shore. Observations, however, show that the young herring are likely to have little difficulty in ascending 200 fathoms before the yolk-sac is exhausted, and that though no one is yet well acquainted with the food of the fry, there can be no doubt about the richness of the surface fauna beyond even the fifty-mile line.

In the Report for 1883, Prof. Ewart called attention to the fact that the German Commission had arrived at the conclusion that the Baltic herring differed sufficiently from the North Sea herring to be worthy of being considered a special variety. It has long been held by fishermen and others that each district has its own peculiar variety. From some 500 specimens examined in 1883, no evidence of the existence of such varieties was found. In order to settle this question finally, Mr. Duncan Matthews has been examining, for a considerable time, samples of the herring captured around the Scottish coast, and now communicates an important paper on this subject. The method of investigation adopted was to take accurate measurements of the length of the head, and of the caudal, dorsal, and anal fins, to note the position of the fins on the body, &c., and, by a comparison of these data with the length of the body, to ascertain the amount of their actual variation, and especially whether these variations were so constant in the herrings of any one or more localities or seasons as to indicate a distinction of races. From this inquiry it seems that there are as large herring now as there were some generations ago, and that, although each district yields large herring, the north-east coast has a slight advantage in this respect over the south-east and west coasts. A table giving the size, &c., of the largest fish examined includes representatives from every fishery district, and shows that there is no practical difference in size between the male and female, nor in the numbers of each of these which were taken. The winter fish are found to be rather larger than those taken in summer, while among the fish commercially termed "maties" there are (1) immature herring, *i.e.* herring which, in addition to being small in size, have undeveloped milts or roes; (2) small herring in all degrees of ripeness up to maturity; (3) small herring which have spawned—small "spent" herring. Hitherto, the size of the fish, rather than the sexual condition, has apparently determined whether the term "matie" should be applied. In the same districts, and even in the same shoals, *large* sexually immature herrings are often found along with *small* ripe, or nearly ripe, herring; hence herring appear not only to vary in size in their fully adult condition, but also to vary in the size at which they reach sexual maturity. It is pointed out that these results, as well as the fact that the undivided ova vary in size in ratio to the size of the fish, are likely to cause considerable variation in the progeny which result from the interbreeding of fish of varying *size* and age. Of the fish caught in the early part of the season, a much larger proportion are immature and small, and probably also younger than is the case later on. The adult fish appear to reach a more advanced stage of ripeness before they approach the spawning-banks. From the measurements made, it is shown that the length of the head varies considerably the extremes being found in herrings of all localities and both seasons, the percentage with the larger size of head' being rather greater among the winter than the summer herring; but this difference, like that of the total length, is considered insufficient to prove a racial distinction. The position of the centre of the dorsal fin in a majority of the winter herrings is anterior to the centre of the body, whereas among the summer herrings a large percentage have it behind the centre. In the immature fish, however, the fin-centre is generally anterior to the body-centre. The anal and pelvic fins show a corresponding difference in position. As regards the pelvic fin, however, this condition is limited to the adult and larger young herring, the pelvic fin being found, like that of the sprat, anterior to the dorsal fin in young herring below 60 millimetres in length. The pectoral fin varies very slightly in its relative position on the winter and summer herring. The relative basal length of both the dorsal and anal fins conveys no indication of racial distinction between the summer and winter fish. The dorsal fin is in all the herrings generally longer than the anal; only about 1½ per cent. of the summer herrings, and 7½ per cent. of the winter, having the anal fin longer than the dorsal. Further details are given respecting the number of fin-rays, keeled scales, circumstances of spawning, &c., but which scarcely affect the question of racial distinction. The inquiry, so far as it has gone, tends to prove that there is no racial distinction between the herrings found in the various localities around the Scottish coast. Judging, however, from the more backward position of the dorsal pelvic and anal fins, the doubtfully smaller head, and the slightly lesser size of the summer herrings, more minute inquiries may indicate a slight difference between the winter and summer herrings.

Mr. Brook reports on the herring-fishery of Loch Fyne and the adjacent districts during 1885, and under his "Ichthyological Notes" gives a short account of the rare fishes met with during the year.

Naturalists and fishermen alike have long felt the absence of accurate information as to the spawning period of fishes. In order to have a basis on which to found further investigations, Mr. Brook has prepared a provisional list of the spawning period of various food-fishes. This list brings out the great lack of accurate information on the subject, but gives an idea of the opinions as to the spawning periods held by fishermen and others around our coast. These opinions are in many cases conflicting, and in most cases they will require to be altered. Prof. McIntosh contributes an account of the work undertaken at St. Andrews since the last Report, including notes on the eggs and young of fishes studied during the past year. Recently considerable attention has been devoted by Mr. Wilson to the development of the common mussel, and an account of his investigations up to the present time will be found in the appendix. During the summer and early autumn several attempts were made to fertilise the eggs artificially at St. Andrews. The early stages of development were studied from ova obtained in this manner, while the free-swimming embryos were frequently obtained in pools amongst the mussel beds in the Eden and in other localities. In the Board's last Report it was mentioned that Prof. Greenfield had undertaken to investigate the lower organisms met with in some of our more important salmon-rivers. This investigation has been advanced a step, and numerous forms have been isolated and cultivated by the methods previously described.

Mr. Brook and Mr. Calderwood give the further results of examination of the food of these "useful" fishes, the herring, the cod, and the haddock. Mr. Calderwood also sends notes on the Copepods of Loch Fyne, and on the Greenland shark; Canon Norman reports on a Crangon, some Schizopoda, a member of the order Cumacea, new to, or rare in, the British seas; Dr. Stirling, on red and pale muscles in fishes, and on economic products from fish and corresponding vegetable products; Mr. Haliburton, on the blood of *Nephrops norwegicus*; Dr. John Gibson, on physical observations made for the Fishery Board in the Moray Firth during the autumn of 1883.

Ten plates accompany the appendix. It is greatly to be regretted that the Board has not yet been able to survey some of the fishing-banks, more especially those which are supposed to extend along the western shores of the Hebrides, and that the part of the Report dealing with scientific work is not published separately.

THE ELECTRIC CHARGE ON THE ATOM

ALTHOUGH considerable attention has been given of late to electrolysis and the subjects connected therewith by English chemists, more especially since the Helmholtz Faraday Lecture of 1881, yet some of Prof. Helmholtz's deductions from Faraday's experiments have been curiously neglected.

I refer more especially to the bearing of the facts on the true nature of valency, and I purpose in this paper to point out one or two fairly obvious consequences which follow from the results of Faraday's researches, but which have not, I believe, been stated before.

Prof. Helmholtz has shown that it follows from Faraday's experiments on electrolysis that while a monovalent atom carries to the electrode one charge of electricity a divalent atom carries two charges of electricity. For instance, when we electrolyse potassium chloride, we have each potassium atom delivering a charge of electricity at the one electrode, and each chlorine atom delivering an equal charge of electricity at the other electrode, all monovalent atoms, carrying with them an equal charge of electricity, which we may call the unit charge.

When, however, we electrolyse magnesium chloride, we have two atoms of chlorine set free for one of magnesium, and consequently while each chlorine atom carries its unit charge with it, the magnesium atom carries two units of electricity to the electrode. In fact electrolysis proves that differences of valency mean differences in the electrical charge on the atom. All this is so familiar to us now that I have perhaps repeated it at unnecessary length.

But we have many elements which vary in valency. For instance, copper is capable of forming two series of compounds, in one of which it is monovalent, and in the other divalent, that is, in one of which the copper atom carries one unit charge of electricity, and in the other carries two units of electricity.

We are able, then, under certain conditions to alter the electrical charge on an atom, increasing it by some simple multiple.

There are therefore a special group of chemical reactions, such as the oxidation of the cuprous salts, in which we have not merely combinations between two or more substances, or ordinary double decomposition, but in which, besides such changes, an additional electrical charge is given to, or removed from, an atom. I think it follows from this that all such reactions are of very special interest, and deserve careful study.

For instance, take the case of the saturation of an olefine by chlorine. We must look on this reaction from one of two points of view. Either on the addition of chlorine an additional charge is supplied to the carbon atom, in which case by-products of less saturation are probably formed; or the carbon atom is already fully charged, in which case the double bond is not merely a shorthand statement of a possible reaction, but expresses a physical fact.

There is also another point worthy of note in connection with this addition of electricity to the atom. If we take the case of the two copper chlorides—cuprous and cupric chloride—we find that their heat of formation per chlorine atom is not very different. Now it is well known that the heat of formation of a salt approximates to the heat of formation as calculated from the electromotive force developed when that salt is formed in a voltaic cell.

To put this in other words, we can obtain from the heat of formation of cuprous chloride, or of cupric chloride, an approximate calculation of the difference of electric potential between the copper atom and the chlorine atom in the two salts.

Now, as already stated, the heat of formation per chlorine atom is nearly the same; that is, the difference of potential between the copper and chlorine is nearly the same in both salts. What follows from this?

It follows that, in doubling the electric charge on the copper atom, the potential is not also doubled. This means, therefore, that the capacity for electricity of the atom is increased at the same time. This conclusion is not quite certain, as our information is still too scanty on the actual differences of potential in the case of these two salts; and, further, we do not know what fraction of it belongs to the chlorine atom; but, on the whole, the facts we have point to the above conclusion, and it is at any rate a subject well worthy of study to determine whether the capacity of the atom for electricity can vary or not.

Passing from this, I wish to point out another very obvious but nevertheless important deduction to be made from the facts of electrolysis.

We have recognised that the difference between monovalent and divalent copper consists in the doubling of the charge upon the atom. This again may be due to some profound change in the atom itself, but it is at any rate the obvious and marked distinction; we have copper in both cases, but double the electrical charge in one case over that in the other.

If we searched among the elements, could we find two series of salts more completely different in their nature and properties than the cuprous and cupric salts?

I venture to say that, if we did not know we could derive the same element from both, we should assume them to be derived from two different elements, and assign them very different places in Mendelejeff's table. Many other examples of the same thing will occur to everybody, namely, that alteration of the electrical charge on the atom is accompanied by profound alteration in the nature of its compounds, and is therefore probably the cause of this alteration.

Up to this point I think my deductions are fair and obvious deductions from the facts of electrolysis. I wish now to suggest a possibility, I can call it no more, which if true will considerably alter our views of the facts of chemistry. We have found the importance of alterations of electrical charge in altering the properties of an atom as shown in its compounds.

We already believe that variations in atomic weight are closely allied with the variations in the properties of the atom as shown in its compounds.

Are there, then, two things which condition the chemical properties of an atom, or is there only one?

Let us look again for an instant at the facts of electrolysis, and let us take the electrolysis of hydrochloric acid as our example.

At present we state the facts thus:—Every molecule of hydrochloric acid consists of one atom of chlorine and one atom of hydrogen, the chlorine atom weighing 35·5, the hydrogen atom weighing 1. On passing a current, each molecule is split into these two atoms, each atom carrying a unit charge of electricity.

Is it not just possible that we may some day state the facts thus:—A molecule of hydrochloric acid consists of one molecule of hydrogen weighing 1 combined with 35·5 molecules of chlorine each weighing 1. On electrolysis, the chlorine atoms are split from the hydrogen atom, the chlorine atoms each carrying unit charge of electricity, and the hydrogen atom carrying 35·5 charges of electricity.[1]

If this is the truth, then all the atoms of the elements are of the same weight, and probably are made of the same "stuff," and we have two, and only two, things which condition the properties of the atom—namely, its electrical charge and its electric potential, and Mendelejeff's table becomes a statement of the periodic relationship between these.

In suggesting this vague possibility, I do not wish to obscure the first part of the paper, which consists, I believe, of perfectly legitimate deductions from the facts of electrolysis.

I have purposely avoided giving many examples, as I have been dealing with such familiar and common-place chemical reactions that plenty of examples will at once occur to every reader; and sufficient has, I think, been said to show at any rate the importance of experimental inquiry into this subject, and the probability of considerable modifications of our views of chemical facts in the near future.

The new way of looking on valency, which we owe to Prof. Helmholtz, may, as I have already pointed out, completely alter our conception of the nature of an unsaturated carbon compound, and of the process by which saturation takes place; and probably as investigation proceeds in this department it will become necessary to re-dissolve our chemical facts and crystallise them out in completely new mental concepts, while doubtless the ideas associated with the graphic formula pass away and leave not a wrack behind.

A. P. LAURIE

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[1] No one need quibble about the 35·5.

MUSIC AND MATHEMATICS

YESTERDAY afternoon meeting at a friend's house a lady visitor to Oxford who was to sing that evening at one of the hebdomadal concerts in Balliol College, and the conversation happening to turn on the gifted mathematical lady Professor in the University of Stockholm, my thoughts shaped themselves, as I was walking home, into the following lines, which, if likely to interest any of your readers, I shall be happy to see appear in the world-wide-diffused columns of NATURE.

New College, November 15 J. J. SYLVESTER

SONNET

To a Young Lady about to sing at a Sunday Evening Concert in Balliol College

Fair maid! whose voice calls Music from the skies
Weaving amidst pale glimpses of the moon
Tones with fresh hues of glowing fancy strewn
And soft as dew that falls from pitying eyes–
Let from their virgin fount those accents rise
That bid sad Philomel suspend her tune,
Thinking the lark doth chant his lay too soon–
Whose else that trill which with her own note vies!
To her whose star shines bright o'er Maelar lake
And thee who beautifi'st glad Isis' shore
Grant! I one joint harmonious garland bind:
Thou canst with sounds our senses captive take–
She the true Muse, fond poets feigned of yore,
Strike Heaven's own lyre, Nature's o'erruling mind.

NOTES

MR. HAROLD B. DIXON has been appointed Professor of Chemistry and Director of the Chemical Laboratories at Owens College, Manchester.

THE *Oxford Magazine* announces that Prof. Burdon Sanderson and Mr. Gotch are going to spend their Christmas vacation at Arcachon, where there is awaiting them a large tank full of torpedoes. It looks forward with interest to the publication of the results of the Oxford physiologists' holiday, remarking that "to the research will be added the pleasing excitement of danger; for if incautiously handled these torpedoes will give the physiologist a shock, compared with which the agonies of scores of vivisected rabbits are as nothing." Of course this is not true.

Mr. William Fawcett, one of the assistants in the Department of Botany in the British Museum, has been appointed by the Secretary of State for the Colonies to the post of Director of the Public Gardens and Plantations in Jamaica.

The Uralian Society of Natural Sciences is arranging to hold a Scientific and Industrial Exhibition of Siberia and the Ural Mountains in 1887, at Ekaterinburg. The Exhibition promises to be of great scientific interest, including sections of geology, botany, zoology, anthropology, and archæology. The anthropology especially will be of the most comprehensive character. It will include a certain number of families of Bashkirs, Kirghizes, Vogones, Ostiaks, Samoiedes, and other half-civilised peoples of Siberia, with their dwellings, and appliances for hunting and fishing, besides models, figures, costumes, &c. There will also be a large collection of prehistoric objects. The grounds around the Exhibition building will be utilised to give a fairly accurate idea of the arborescent vegetation of the countries represented. Delegates from foreign Societies intending to visit the Exhibition will be accorded special travelling privileges, and be received with the greatest hospitality. The Exhibition will be open from May 27 to September 27. The President of the Society is General Ivanoff, and the President of the Exhibition Committee M. A. Miclowsky.

Mr. Arthur Grote died on the 4th inst. at his residence in Ovington Square. He was born in 1814. He was a Fellow of the Royal Society and also of the Linnean Society. Mr. Grote wrote a number of papers on subjects connected with botany and natural history, and contributed an introduction to Hewitson's "Descriptions of New Indian Lepidopterous Insects in the Atkinson Collection." He was engaged for many years in Her Majesty's Indian Civil Service.

Mr. W. Galloway writes with reference to the recent explosion at Elemore Pit :—"Elemore Pit is one of the Heeltan group of collieries near Newcastle-on-Tyne, which are accounted to be amongst the best and most carefully managed mines in this or any other country. It is under the direct supervision of Mr. Lindsay Wood—a member of the late Royal Commission on Accidents in Mines. The workings are dry and dusty; no accumulation of gas was known to exist anywhere; a shot is supposed to have been fired at the moment the explosion took place, for shot-firing was going on. The explosion appears to have been confined to the in-take air-ways, and to have shot up the downcast shaft, or, in other words, to have traversed a region filled with fresh air without any admixture of fire-damp. These features of this and other similar explosions of late appear to militate somewhat against the views expressed in the Report of the Royal Commission on Accidents in Mines, to the effect that coal-dust alone was a comparatively harmless agent in the absence of fire-damp."

The following are the arrangements for the lectures before Easter at the Royal Institution :—Prof. Dewar, six lectures (adapted to a juvenile auditory) on "The Chemistry of Light and Photography"; Prof. Gamgee, eleven lectures on "The Function of Respiration"; Prof. Rücker, five lectures on "Molecular Forces"; Prof. Max Müller, three lectures on "The Science of Thought"; Mr. Carl Armbruster, five lectures on "Modern Composers of Classical Song"; and Lord Rayleigh, six lectures on "Sound." The following are the probable arrangements for the Friday evening meetings before Easter :—Sir William Thomson, on "The Probable Origin, the Total Amount, and the Possible Duration of the Sun's Heat"; Mr. W. Baldwin Spencer, on "The Pineal Eye in Lizards"; Mr. Edwin Freshfield, on "Some Unpublished Records of the City of London"; Mr. E. B. Poulton, on "Gilded Chrysalides"; Mr. W. Crookes, on "Genesis of Elements"; Capt. Abney, on "Sunlight Colours"; Mr. V. Horsley, on "Brain Surgery in the Stone Ages"; Archdeacon Farrar, on "Society in the Fourth Century A.D."; Mr. G. J. Romanes, on "Mental Differences between Men and Women"; and Lord Rayleigh, on "Colours of Thin Plates."

An Edinburgh Correspondent writes :—"Mr. Romanes has just delivered his first course of lectures on the Philosophy of Natural History to a large and appreciative audience, including students from all the Faculties in the University of Edinburgh."

The Minister of French Postal Telegraphy has sent to Brussels a delegate to arrange for establishing a telephonic line between Paris and that city. The price for the use of the telephone will be five francs for a period of five minutes. It is the first step on record towards an international telephonic system.

A remarkable fire-ball was seen at Stonyhurst College, Blackburn, on December 4, at 9.16 p.m. Although the moon was shining brightly, it lit up the sky like a brilliant rocket. Its course was from 27 Lyncis to θ Gemini, and as it advanced it left a fine streak behind it. Its colour was a bluish-white. At first it moved swiftly, then more slowly, and, before vanishing, burst into several fragments. There was no streak left where the meteor burst, but only in the first part of its course. It was like a horse-tail cirrhus, the bushy portion surrounding the star 27 Lyncis, and thence extending in a narrower streak about 3°. It remained visible for one minute and a half, the part last to fade being that about 27 Lyncis.

We have received the last number (part 2, vol. iv.) of the *Proceedings* of the Liverpool Geological Society. The presidential address by Mr. Mellard Reade, "On the North Atlantic as a Geological Magazine," is a continuation of the line of investigation sketched out by him in his previous address, which was entitled "Denudation of the two Americas." The papers deal mainly with the geology of Liverpool and the neighbourhood, especially North Wales. A paper by the Secretary, Mr. W. Hewitt, "On the Topography of Liverpool," must be specially interesting and instructive to those acquainted with the present geography of that great city.

The active volcano, Asamayama, appears to be attracting particular attention just now in Japan, probably because it is the loftiest mountain in the country which is in a constant state of activity, and also because it is the nearest to the capital, and is situated in a district long famous for its health resorts. A few weeks since we referred to an anonymous account of the crater, published in the *Japan Weekly Mail*, but a much more careful sketch of it is given by the Japan Correspondent of the *Times* in a letter published recently in that journal. The roar of the volcano, on approaching the edge of the crater, he describes as not unlike the noise produced by the passage of a train across a bridge under which one is standing. There was no shaking, however, but loud hissing and bubbling constantly proceeded from numberless vapour-jets in the inner face of the crater-wall, from its rim downwards. The crater is a rough oval in shape, but the estimates of its size are most conflicting. The Japanese give the circumference as four miles, but this is simply a wild guess. A German explorer set down the diameter at 1100 yards, and an English mathematical professor put it at only 200 yards, "divergences that will illustrate the mental confusion to which some men are liable when in the presence of dread natural phenomena." The writer himself estimated the circumference at 1056 yards, by walking round the windward half of it—it was impossible to pass through the vapours on the lee side—which was accomplished in six minutes, at the rate of about three miles an hour.

On the very interesting question of the depth of the crater—that is, the depth from the mouth to the surface of the molten matter—opinions vary almost as hopelessly as on the size. No doubt the "vast clouds of the most pungent sulphurous steam," which are described as rising swiftly out of the caldron, render exact observation difficult. The *Times* Correspondent speaks of catching glimpses of the crater-wall at depths which a very moderate estimate would place at 300 feet. But the gradual convergence of the cavity apparent at this depth forbids the acceptance of the enormous profundity for which some visitors have contended, and suggests that the depth can hardly much exceed 500 feet. After a weird description of the appearance presented to the spectator by the volcano at work, the writer concludes by remarking that the present crater is apparently the youngest and innermost of three. Further down on the south-west side are to be seen, along with numerous fissures of unfathomable depth, remains which point to the existence of two former craters, concentric and of large dimensions, and separated from one another by a considerable interval. Possibly the existing cone was formed by the great eruption of 1783.

A TELEGRAM from New York of December 2, states that eight slight shocks of earthquake are reported from Summerville, one severe shock from Columbia, and two slight ones from Charleston. No damage was caused. Dr. Forel writes that earthquakes were felt in Switzerland on November 25 at 3h. at Pontresina and Bernina (Grisons), and again at 3h. 58m. (both Greenwich times) at Pontresina.

ACCORDING to the *Ceylon Observer*, Mr. C. Stevens, a naturalist, has returned to Colombo from a most successful and interesting sojourn amongst the Veddahs, whose district he has thoroughly explored, and with whom he was enabled to establish a closer intimacy than any European ever did before. He has been able to clear up a good many dubious points relating to the manners, customs, and religious beliefs of these veritable wild men of the woods. He has succeeded in obtaining several perfect skeletons, and a number of skulls.

THE *London and China Telegraph* states that a Folk-Lore Society has been established in the Philippines, at the prompting of a Society for the study of folk-lore in Spain. The archipelago certainly presents a wide field for investigation and inquiry in this respect, on account of the diversity of native races inhabiting it. The survivals in the shape of traditions, customs, and observances amongst the primitive tribes still to be found in the inaccessible interior of many of the islands may be expected to throw much light on the early history of the people, and on the origin of many superstitious practices common in more civilised lands.

THE Report of the Public Free Libraries of the City of Manchester, while expressing the deep regret of the Committee at the loss of Sir Thomas Baker, their chairman for nearly twenty-five years, is at the same time a testimonial to the ability and judgment with which the work under his care has been carried on. Additional libraries, a tenfold increase in circulation since the two first of them were opened, and over 4000 volumes withdrawn this year as *worn out*, are proofs of the earnestness of this work. Nor does the increase seem likely to cease, for the extension of the time of keeping open the reading-rooms till 10 o'clock, although it adds to the already long hours of those engaged in their management, is sure, we think, to increase their counter-attraction to the public-houses, and to bring up the number of visitors annually to the libraries to three millions. Two recently incorporated districts also have requested that equal advantages may be extended to them, and help in carrying this out has been liberally supplied by independent public bodies. A remarkably large proportion of books are taken out to be read in the reading-rooms. Boys especially avail themselves of these rooms on a Sunday, nearly twice as many of them attending then as on a week-day; a direct reversal of the practice of other classes. The success of Manchester is the more marked that so moderate a proportion of fiction is supplied to its readers.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♂ ♀) from India, presented by the Countess Dowager of Lonsdale; a Mona Monkey (*Cercopithecus mona* ♀) from West Africa, presented by Miss Bashall; a Domestic Sheep (*Ovis aries*, var.) from West Africa, presented by Sir Albert Kaye Rollitt, F.Z.S.; a Grey-striped Mouse (*Smithus vagus*) from the Tatra Mountain, presented by Dr. A. Wryesniowski; a Poë Honey-eater (*Prosthemadera novæ-zealandiæ*) from New Zealand, presented by Capt. B. J. Barlow, s.s. *Taimu*; a Blue-fronted Amazon (*Chrysotis æstiva*) from Brazil, presented by Miss Joachim; two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, presented by Dr. E. B. Parfitt; a Cerastes Viper (*Vipera cerastes*) from Egypt, presented by Mr. J. H. Leech, F.Z.S.; a Beisa Antelope (*Oryx beisa* ♂) from North-East Africa, a Rough Fox (*Canis rudis*) from Guiana, purchased; a Red Kangaroo (*Macropus rufus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

CORRECTIONS TO REFRACTION TABLES.—Prof. Cleveland Abbe, in a short note to the *Astronomische Nachrichten*, calls attention to the fact that the reading of the mercurial barometer which is used in the refraction-formula as an index to the density of the air is not a true index to the pressure controlling that density until it is corrected for the effect of the variations in gravity. The correction is accomplished by adding one more factor, g, for gravity, when the formula becomes—

$$R = \alpha \tan z \left(\frac{1 - 0{\cdot}00259 \cos 2\phi}{1 - 0{\cdot}00259 \cos 2\phi_0} BT \right)^{A} \gamma^{\lambda},$$

where ϕ is the latitude of the observer, and ϕ_0 of the station for which the tables were computed. Prof. Cleveland Abbe considers that the omission of this correction for gravity may partly explain the origin of small systematic differences in the declinations of different star-catalogues, though such differences, so far as they are due to refraction, must also be caused by local irregularities in the distribution of pressure and temperature, which produce effects equivalent to slight inclinations of the horizontal planes of equal density. The systematic changes in his distribution, due to change of season, must introduce an annual variation in refraction similar to the effect of parallax, and it will occasion a difference in the refractions north and south of the zenith, which may often attain an appreciable amount.

COMET FINLAY (1886 *e*).—The following ephemeris by Dr Krueger, for Berlin midnight, is in continuation of that given in NATURE of November 25 (p. 85):—

1886		R.A.			Decl.		log *r*	log Δ	Brightness
		h.	m.	s.	°	′			
Dec.	10	22	2	49	13	25·6 S.	0·0074	9·8941	3·1
	14	22	23	51	11	9·7	0·0142	9·8909	3·0
	18	22	44	58	8	46·0	0·0221	9·8901	2·9
	22	23	6	0	6	16·7	0·0309	9·8917	2·8
	26	23	26	49	3	44·6 S.	0·0404	9·8958	2·6

The brightness at date of discovery is taken as unity.

COMET BARNARD (1886 *f*).—This object is now visible to the naked eye, and is at its brightest. As it is now visible in the early evening, it should be frequently observed. The following ephemeris by Dr. Aug. Svedstrup, for Berlin midnight (*Astr. Nachr.*, No. 2756), is in continuation of that given in NATURE of November 25 (p. 85):—

1886		R.A.			Decl.		log *r*	log Δ	Brightness
		h.	m.	s.	°	′			
Dec.	11	17	6	7	16	20·9 N.	9·8266	0·0004	24·7
	16	17	54	19	13	43·9	9·8212	0·0300	22·1
	21	18	34	18	10	32·2	9·8266	0·0679	18·0
	26	19	6	32	7	13·7	9·8421	0·1084	14·0
	31	19	32	32	4	5·2 N.	9·8652	0·1478	10·5

The brightness at date of discovery is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 DECEMBER 12-18

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 12

Sun rises, 7h. 59m.; souths, 11h. 53m. 58·0s.; sets, 15h. 49m.; decl. on meridian, 23° 6′ S.: Sidereal Time at Sunset, 21h. 14m.

Moon (one day past Full) rises, 16h. 34m.*; souths, 0h. 29m.; sets, 8h. 27m.; decl. on meridian, 18° 57′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	6 15	10 43	15 11	18 3 S.
Venus	8 12	12 4	15 56	23 30 S.
Mars	10 10	14 6	18 2	23 1 S.
Jupiter	3 14	8 26	13 38	10 7 S.
Saturn	18 6*	2 9	10 12	21 33 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Dec.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
14	3 Cancri	6	1 42	2 46	100 237
14	B.A.C. 2731	6½	6 54	near approach	207 —
14	54 Cancri	6½	21 26	22 9	85 181

Dec. h.
13 ... 6 ... Mercury stationary.
13 ... 17 ... Saturn in conjunction with and 2° 59′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Dec. 13,	1 25	*m*
			,, 18,	1 5	*m*
T Arietis	2 42·0	17 2 N.	,, 16,		*m*
S Tauri	4 23·0	9 42 N.	,, 18,		*M*
R Leporis	4 54·4	14 59 S.	,, 12,		*M*
S Cancri	8 37·4	19 27 N.	,, 17,	2 24	*m*
W Virginis	13 20·2	2 47 S.	,, 15,	21 0	*M*
δ Libræ	14 54·9	8 4 S.	,, 16,	4 50	*m*
U Coronæ	15 13·6	32 4 N.	,, 13,	22 15	*m*
β Lyræ	18 45·9	33 14 N.	,, 15,	2 30	m_2
δ Cephei	22 24·9	57 50 N.	,, 16,	4 50	*m*
			,, 17,	19 20	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers

Moonlight interferes with meteor observation during the early part of the week, which is also less fruitful of meteors than are the first few days of the month. Amongst the radiants which have supplied meteors at this season are one in the constellation of the Lynx, R.A. 108°, Decl. 63° N., and one in Quadrans, R.A. 221°, Decl. + 53° N.

THE LAW OF STORMS IN THE EASTERN SEAS[1]

I. In the Eastern seas the earliest signs of a typhoon are clouds of the cirrus type—looking like fine hair, feathers or small pale white tufts of wool—travelling from the east or thereabout, their direction backing towards the north, a slight rise in the barometer, clear and dry but hot weather, and light winds. This fine weather lasts for days, and the existence of a typhoon at a great distance contributes therefore to the safety of ships at sea,—a fact that is not sufficiently appreciated by mariners.

The cirrus clouds, which frequently assume fantastic shapes, make their appearance within 1500 miles of the centre of a typhoon, the barometer is generally rising beyond from 600 to 1000 miles of the centre, and the mean of the twenty-four hours' temperature rises in Hong Kong above 81°.

A swell in the sea is noticed within from 300 to 500 miles of the centre, but this depends greatly upon the situation of the nearest land. Halos round the sun and the moon, phosphorescence of the water, and also glorious sunsets appear to be frequently noticed before typhoons.

Within about 800 miles of the centre the sky is generally half covered with cumulus clouds, above which cirro-cumulus are usually seen. South and south-west of the centre, thunderstorms and cumulo-stratus clouds are observed. On approaching nearer to the centre the cloudiness increases, the temperature falls in consequence, and the mercury begins to descend in the barometer. Then the air becomes oppressive from the increasing dampness, a slight haze is observed during the morning hours, and the sky presents a threatening and vaporous appearance. Within 300 miles of the centre the temperature falls quickly owing to the cumulus, roll-cumulus, or nimbus clouds, with which the sky is nearly completely overcast. And meantime the wind has risen and blows generally with the force of a strong breeze about 300 miles from the centre. But this depends also upon the bearing of the centre, the wind being usually strongest in the right hand semicircle. Within 150 miles of the centre the sky is densely overcast with nimbus clouds accompanied by heavy rain, and within 60 miles it generally pours down in torrents, while the wind blows so hard that no canvas can withstand it; but there is no thunder and lightning. The temperature at sea is frequently about 76°, and on shore about 78°.

Within from 2 to 15 miles of the centre the wind either calms down or blow only moderate breezes, and the sky clears, being now covered only by very light clouds. The sea is as a rule mountainous, but in some reports it is stated that the sea had calmed down to some extent when the wind fell. Quantities of sea-birds, and near land also butterflies and other insects, cover a ship situated in the bull's eye of a typhoon. It is possible that the central calm does not quite accurately coincide with the centre of the typhoon.

The angle between the direction of the wind and the direction of the radius (the straight line between the observer and the centre of the typhoon) is, on an average, between 10° and 25° latitude, 43° in front of the centre and 53° behind the centre; between 33° and 35° latitude, 65° in front and 85° behind; and between 10° and 35° it is about 49° in front and 62° behind the centre. The angle appears to be smaller near the shore for off-shore winds, and far out at sea the difference between the angle in front of and behind the centre appears to be small. The following rule for finding, on board ship in the China seas, the bearing of the centre of a typhoon is, therefore, approximately correct: Stand with your back to the wind, and you will have the centre on your left side, but 3 points in front of your left hand; *i.e. the centre bears about* 11 *points from the wind.* If your ship is in a very low latitude the centre may lie as much as 4 points in front of your left hand, *i.e.* bear 12 points from the wind, and if you are in a high latitude it may bear only 9 points from the wind. Once the wind has reached the force of a strong breeze, the average angle between the wind and the direction of the centre does not appear to change at all, but the wind, which blows in great gusts in a typhoon, may oscillate to both sides of the true value. There does not appear to be any foundation at all for the belief that the wind near the centre blows in circles round the centre. To act according to this rule might prove disastrous to a ship experiencing a typhoon.

Very low clouds in a typhoon move with the wind, but if the clouds are high they are frequently seen to move in a different manner, and the following rule may then occasionally be of use: If right in front of the centre, stand with your back towards the direction whence the clouds are coming, and you will have the centre from 1 to 2 points in front of your left hand; and if straight behind the centre you may have it a point or two to the left of the direction in which you are looking.

Once the bearing of the centre has been ascertained, the master of a vessel in a typhoon requires to know in which semicircle, looking in the direction towards which the typhoon is moving, he is situated: If *in the right hand semicircle, the wind will veer, i.e.* shift with the sun; and if *in the left hand semicircle, it will back, i.e.* shift in the opposite direction. But this rule is strictly applicable on board of a vessel only when hove-to, or at any rate proceeding at a slower rate than the typhoon. For a vessel moving at a faster rate than and in the same direction as a typhoon, the rule may be reversed. In case of doubt it may therefore become advisable to heave-to in order to be quite sure of the semicircle in which you are situated. But we have seen that the wind moves in spirals towards the centre, and it is therefore dangerous to lie-to in a typhoon, particularly before you are sure that the centre is past. Vessels near the coast of China, or in the Formosa Channel, generally seek refuge in the nearest typhoon harbour indicated in the Directory.

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[1] By Dr. W. Doberck, Hong Kong Government Astronomer. Reprinted from the *Hong Kong Telegraph.*

The wind shifts faster the nearer the centre you are. If the barometer falls rapidly and the wind does not change its direction, and when the gusts continue to increase in force, your ship is in danger of entering into the central calm of the depression with its mountainous and confused seas, which is by all means to be avoided, as it is the high cross seas that do the most damage, and not the force of the wind. When once you are caught in a typhoon you should make no sail, except what may be necessary to steady the ship, till the gusts continue to decrease in force and the barometer has risen for some time. Very deceitful lulls are reported to occur during the raging of a typhoon. The master of a sailing-vessel is said to have put up topgallant sails after getting into the central calm. Of course he could have had no reliable barometer on board.

In storms encountered in higher latitudes, where the incurvature of the wind is not so great as in a tropical hurricane, the right-hand semicircle is termed the dangerous semicircle, as a ship running before the wind is in more danger of crossing the path of the storm in front of the centre and perhaps be overtaken by it; but in a typhoon there is not much to choose between the semicircles. A dismasted ship is in danger of being carried into the centre from any quarter.

However, the right-hand semicircle is also in a typhoon generally more dangerous than the other, both with regard to the risk of crossing the path in front of the centre, and also, as remarked above, with regard to the force of the wind and consequent greater sea disturbance. A ship experiencing a northerly gale and a falling barometer in the China Sea in the typhoon season is generally in greater danger than another experiencing a south-westerly gale.

When you have ascertained in which semicircle your vessel is situated, you should, if in the right-hand semicircle, keep the wind as long as possible on the starboard tack; and if in the left-hand semicircle, you should run on the starboard tack, or heave-to on the port tack, so as to let the ship come up as the wind backs and run no risk of being taken aback. As explained further on, a typhoon encountered in a low latitude moves so slowly that a steamer or fast sailing-ship has a fair chance of running away from it, but farther north, when the centre proceeds at the rate of thirty or forty miles an hour, it requires careful management even supposing you have ample sea-room.

Typhoons are dangerous on the open sea, but they are still more to be feared in open anchorages or near lee shores. Along the south-west coast of Formosa and elsewhere, a ship must in the south-west monsoon be prepared to run to sea at very short notice, as in some of the harbours you could not lie with any chance of riding out a typhoon. A steamer at anchor should get up steam as soon as the wind rises above the force of a strong breeze, and a sailing-vessel should take down the top-masts. The direction in which the wind is going to shift must early be determined so as to select a sheltered anchorage. If the centre passes very near the anchorage, the berth may have to be changed to the other shore during the lull, before the wind shifts to the opposite quarter.

A ship moored by a single anchor with her head to the wind, will swing with the sun in the right-hand semicircle and against the sun in the left-hand semicircle. If two anchors are dropped, the anchor on the advancing bow should be let go first, therefore a ship in the right-hand semicircle of a typhoon should first drop the port anchor and afterwards the starboard, in order that she may ride with open hawse. And a ship in the left-hand semicircle should first drop the starboard anchor. But ships have to ride with a long scope in a typhoon, and as they are liable to drag the anchors, some prefer to drop the second anchor to veer upon if the first should not hold.

II. The force of the wind and the appearance of the sky do not always furnish a reliable guide to determine how far you are from the centre of a typhoon. The dimensions are different in different typhoons, and near land the strong winds are often so irregularly distributed that in a place near the centre less wind may actually be experienced than at some distance farther away from it. Also the 11-point rule for ascertaining the bearing of the centre fails near some shores if the centre is not near at hand; thus there often blows a steady easterly gale along the southern coast of China when a typhoon is crossing the China Sea, and the gale blows often steady from north-east about the northern entrance to the Formosa Channel when there is a typhoon in a more southern latitude.

The surest of all warnings is furnished by the standard barometer on shore and the compensated aneroid on board ship; you are all right if you can put your vessel on the tack that will keep your barometer rising. But in order to understand the indications of the barometer you will have to keep a regular meteorological register. The master of a vessel who does not look at his aneroid till he is in a typhoon, does not derive half the benefits from his observations that he would have enjoyed had he watched it beforehand. He might perhaps have avoided the weather he is now experiencing, or even have benefited by the favourable winds and sailed round the typhoon. No doubt the time is approaching when underwriters will stipulate that the indications of an aneroid or a marine barometer must be regularly registered on board a vessel insured by them.

On the other hand, it would not be fair to ask the mariners to keep complete meteorological records, such as are kept in the lighthouses out here. Some seamen have a taste for this kind of work and make very useful and fairly accurate observations, but, for instance, the readings of dry and damp bulb thermometers taken on many vessels are of very little use.

The tube of the marine barometer has to be so much contracted to stand the incessant pumping and danger of breakage, that the instrument is sluggish and often reads half an inch or more too high near the centre of a typhoon. Some cheap wooden barometers cannot be registered below a certain height, the cistern being too small to hold the mercury that comes out of the tube. Of course some cheap aneroids are no better, and even a first-class compensated instrument requires to be thoroughly verified, as the scale is never quite correct, but they act nearly as quickly as first-class standard barometers, and for use on board ship the instrument that is quickest in its indications must be preferred. The objection to the use of the aneroid is founded on the fact that its index-correction changes gradually; but then this can be determined and allowed for by reading it off as often as the vessel enters a port, such as Hong Kong, where correct meteorological observations are constantly being made.

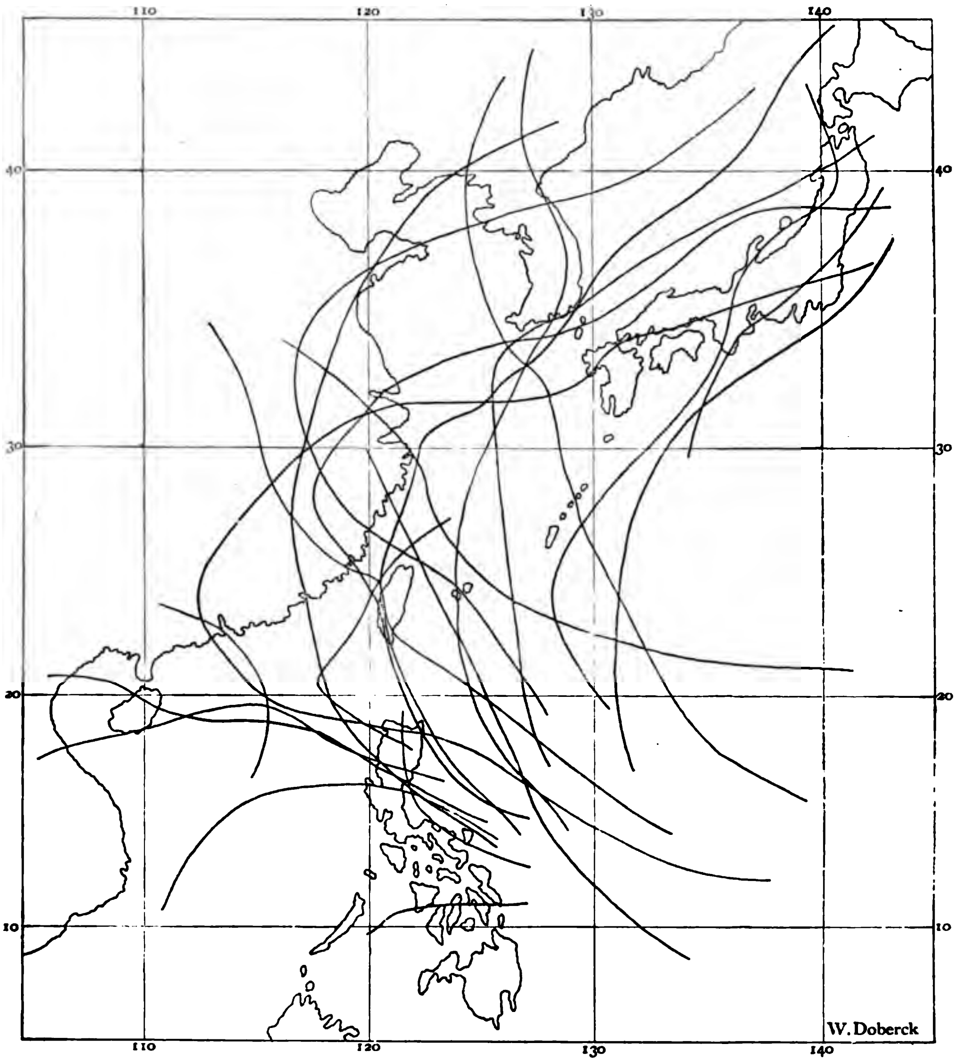
The best hours for making observations are 4 a.m., 8 a.m., &c., up to midnight inclusive. The observations should consist in readings of the aneroid, temperature (this is no use except when the thermometer is placed well forward so as to be exposed to the wind, though in a position sheltered from the sun and the rain), direction and force (0–12) of the wind, direction whence coming of the clouds, amount (0–9) of sea-disturbance, and weather (Beaufort's notation). For further particulars the "Instructions for making Meteorological Observations, prepared for use in China," published in 1883 by the writer, may be consulted.

From 4 a.m. to 10 a.m. the barometer is rising, falling from 10 a.m. to 4 p.m., rising from 4 p.m. to 10 p.m., and falling from 10 p.m. to 4 a.m. It reads highest at 10 a.m. and lowest at 4 p.m. During the approach of a typhoon this regular daily variation may be masked, but it goes on all the same, and must be taken into account when the barometer begins to fall before a typhoon. Thus if it has fallen a certain amount between 10 a.m. and 4 p.m. you must subtract the normal descent between these hours in order to know how much of the fall is due to the approach of the typhoon, and if it were between 4 p.m. and 10 p.m. that it fell, you must add the normal rise for the same purpose.

In many typhoons, the barometer, reduced to 32° Fahrenheit, and to sea-level, does not fall below 28·80 inches. In others it falls as low as 28·50. Lower readings of the barometer appear to be rare, but it has been stated to have fallen much lower. The rate at which your barometer is falling depends upon your approach to the contre, and in consequence upon the rate at which this is travelling. For this reason it is not safe to draw conclusions concerning the amount of wind to be expected from the rate at which the barometer is falling, but to some extent, of course, this may be done. Remember, that, when the barometer has fallen to its lowest reading and begins to rise, you may expect to experience as much bad weather as you have already gone through.

The wind blows from a region where the barometric pressure is higher, towards one where it is lower, being, however, deflected towards the right in a typhoon, and the force of the wind depends upon the difference of pressure between one place and another situated in the direction of the greatest barometric slope or gradient. This is expressed in hundredths of an inch per fifteen nautical miles. Now, the gradient corresponding to a certain force of the wind is somewhat uncertain, particularly when the force of the wind exceeds a whole gale, but on an average a gradient of 0·02 inches in 15 miles corresponds to a force of wind equal to 6 on Beaufort's scale, 0·03 to 7, 0·04 to 8, 0·05 to 9, 0·07 to 10, 0·10 to 11, and where the gradient is above a tenth of an inch in fifteen miles it generally blows with full typhoon force. In low latitudes the gradient occasionally exceeds one inch in fifteen miles.

Curved lines drawn on a map through the places from which the same height of the barometer (reduced) is reported, or between those that report a higher and a lower barometer, are called isobars. The gradient lies at a right angle to the isobar. These are the most important elements in forecasting the weather. Thus during the south-west monsoon the barometer as a rule reads higher over Luzon than along the China coast, the gradient being directed from about south-east towards north-west, indicating southerly winds as prevailing over the China Sea according to the 12-point rule. But when, as occasionally happens in the typhoon season during the south-west monsoon, readings reported from stations along the south-eastern coast of China are higher than those reported from Luzon, the gradient is found to be reversed, being directed towards south-east, thus indicating northerly winds. At such times a typhoon may be expected, and the probability is increased if the barometer is falling in Luzon and rising slowly in Northern China and Japan, and if cirrus clouds have previously been observed to come up from east or north.



Principal Typhoons of 1884 and 1885.

III. Nearly all the typhoons appear to have their origin east or south-east of the Philippine Archipelago, in a part of the ocean south of the high-pressure area that covers the Northern Pacific in the summer season, which part of the ocean is characterised by high sea-surface temperature. Typhoons are sometimes formed in the China Sea, but then they seldom develop much energy, as they usually move quickly northwards and enter the mainland of China or Formosa. Owing to their small dimensions they are easily avoided by such ships as may fall in with them. The sea-disturbance is nothing terrible, and only whole gales of wind were reported in those cases that have been investigated here. If, however, a typhoon of this kind passes northwards up through the Formosa Channel, it soon becomes as formidable as any of those that originate in the tropical Pacific. We have not traced typhoons nearer to the equater than about 9°. But it appears that they may possibly in some cases originate nearer than that to the equator, as hurricanes have been encountered in a lower latitude.

It frequently happens that a vessel encountering a typhoon in, say, 12° north latitude and 135° longitude east of Greenwich, does not experience any strong wind or bad weather till within a hundred miles or so of the centre, and as the typhoons are most violent in that locality, it is very important to look out for the premonitory signs referred to in the first section of this article, taking into account that the dimensions of a typhoon are so small there. On the other hand, they move at so slow a rate that you may run away from them if you are aware of the danger in time, so much more as you may be sure that a typhoon in that locality is directing its course to somewhere about west-north-west or north-west, and most likely in the first-named direction. So it is better to get to the eastward of it. Nearer the Philippine Archipelago the typhoons usually take a more northerly course, moving north-west or north-north-westward. But frequently they continue their course west-north-westward and cross the islands to enter the China Sea. In spring and autumn they have even been found to move westward and turn south-westward after entering the China Sea. But when you are east of the Philippines you should try to get your ship south-east of the typhoon by crossing the path behind the centre, if possible. If you are going northwards, you will then benefit by the fresh south or south-west breezes, taking care not to approach too near to the typhoon, whose progressive motion may not be more than six miles an hour. You will probably have squally and wet weather.

When all the paths of the typhoons that have been investigated by the writer in the course of the last three years are laid down on a map of the Far East, the picture looks much like a fan, the paths, with a few stray exceptions, radiating from the locality referred to above, and running in all directions between west and north, but most of them at first westward and then north-westward. In a higher latitude they generally recurve and pass off to the north-east, after first, of course, having turned northwards. Every typhoon does not recurve; in fact, as stated above, some of them finally disappear in the China Sea after turning to the south-west. The others recurve between 20° and 40° latitude, and 115° and 130° longitude. The Middle Dog Lighthouse is situated in the centre of the region of recurvature.

The normal path is therefore, roughly speaking, a parabola, whose axis lies from east to west, and whose apex is turned westward and lies within the region indicated above. But each path individually is anything but a regular parabola, and the deviations are evidently due to the influence of the coast-line of the mainland of Asia and to the mountainous islands (especially the high mountains of Formosa) as well as to the prevailing winds. For there is no doubt that the progression of a typhoon is the effect of the wind prevailing at the time, not necessarily at the surface of the earth, but at a somewhat higher level in the atmosphere, which agrees with the direction of the clouds, that have, as explained in the first section of this article, been found to move nearly straight towards the centre in the posterior semicircle. If, however, the wind at the surface of the earth is strong, it is at times plainly seen to blow the typhoon before it. The typhoons do not appear to move south-westward in the China Sea except when the north-east monsoon is strong, and in the summer of 1885, when the south-west monsoon was strong, most of the typhoons moved northwards while yet to the east of Formosa. This is then the reason why the typhoons depend upon the season of the year. They are likewise deflected from their previous path when exposed to strong winds blowing out of open channels, such as the Formosa or Corea Channels, in which case the speed of their progress is sometimes abruptly increased.

The average rate of progress of the centre of a typhoon in 11° latitude is 5 miles an hour. In 13° it is 6½, in 15° it is 8, in 20° it is 9, in 25° it is 11, in 30° it is 14, and in 32½° latitude it is 17 miles an hour. The rate of progress does not vary perceptibly in case of typhoons south of 13° latitude, so it is well for masters of vessels to know this, but it is more variable the farther north you go. In 32½° latitude it ranges between 6 and 36 miles an hour, so that you cannot at all be sure that a typhoon, which you may happen to be near, will travel at anything like the average rate of progress in that latitude.

In "Observations and Researches made at the Hong Kong Observatory in the year 1884," the writer suggested the division of typhoons into four classes according to the paths which they usually follow. Of course abnormal instances, such as for instance are presented by the typhoons that originate in the China Sea, occur occasionally in China as well as elsewhere, but they are comparatively rare.

Typhoons of the first class occur at the beginning and the end of the typhoon season. They cross the China Sea and travel either in a west-north-westerly direction from the neighbourhood of Luzon towards Tonquin, passing south of or crossing the Island of Hainan, or, if pressure is high over Annam, they travel first westward and subsequently south-westward. They can generally be followed for between five and six days.

Typhoons of the second class are the most frequently encountered, and their paths can be traced farthest. They generally travel north-westward while in the neighbourhood of Luzon, and either strike the coast of China south of the Formosa Channel, in which case they as a rule abruptly lose the character of a tropical hurricane, recurve in the interior of China, re-enter the sea somewhere between Shanghai and Chefoo (thereby regaining some of their past violence), pass across or near to Corea, and are finally lost sight of in their motion towards about east-north-east; or they pass up through the Formosa Channel, recurve towards north-east, and pass along the coasts of Japan; or they may strike the coast of China north of Formosa. Typhoons following the latter path originate further east of the Philippines than the others. They either continue their motion north-westward, in which case they are soon lost, or recurve and pass north-eastward near Corea. Typhoons of the second class occur from June to September inclusive, but are most common in August and September. It appears that a third of the typhoons belong to this class. They can be followed on an average 7 days, or rather between 5 and 12 days.

Typhoons of the third class are probably the most numerous of all, but are not encountered so frequently as the typhoons of the second class, and therefore the existence of a typhoon of this class is sometimes only suspected, although it of course influences the weather along the eastern coast of China through the fine weather area with which it is surrounded. They pass to the east of Formosa, travelling northwards. After recurving, they generally pass near Japan, but sometimes a typhoon of this class continues to move north-north-westward and does not recurve till west of Corea. They prevail in the same season as typhoons of the second class, and may be traced on an average during 7 days, or more correctly between 3 and 12 days. A typhoon of this class frequently follows after one of the second class. It is a well-known fact that depressions are attracted towards places which have just been traversed by a depression.

Typhoons of the fourth class pass south of Luzon, travelling westward, or first in this direction and then south-westward. They occur at the beginning and the end of the typhoon season, while the north-east monsoon is strong, namely in April and late in autumn, but are very rare. They are said to be more violent in autumn than in spring. Existing in so low a latitude, their dimensions are, of course, very limited. The writer has not been able to follow them for more than a day or two.

The number of typhoons that are known to have occurred in each month of the year, expressed in percentages of the total number of typhoons, is as follows:—January 2, February 0, March 2, April 2, May 5, June 5, July 10, August 19, September 27, October 16, November 8, and December 3. These figures prove that typhoons are most frequent during the month of September, but they also show that, strictly speaking, there scarcely exists a well-marked typhoon season. On an average there are 15 typhoons every year, but typhoons in different years exhibit some variations.

IV. The writer on his arrival in the colony in 1883 found that meteorological observations were received from a few of the Treaty ports, &c., and were published in the local papers; and seeing that these returns would only have to be corrected and reduced, as well as slightly extended, in order to be of great value to the shipping, he took upon himself to effect this. Subsequently, as the official work of the Observatory was fully started, he would have had to give up this purpose had not the Government decided to support it. Thus originated the *China Coast Meteorological Register*, which is published daily from here. It contains, at present, observations of the principal meteorological elements, which are received through the co-operation of the great telegraph companies from Manilla, Bolinao (Luzon), Haiphong, Hong Kong, Amoy, Foochow, Shanghai, Nagasaki, and Vladivostock, but the number of the stations might with advantage be extended. It gives also information about the weather prevailing in the Far East, and more or less rough intimations concerning such typhoons as happen to be indicated by the telegraphic returns, as well as by local observations. Subsequently more or less extensive monthly meteorological returns are received from about fifty land stations in the Far East, and the examination of the log-books of ships calling at this port, as well as observations received from commanders of men-of-war and masters of vessels trading in these seas, furnish a perhaps unequalled amount of material for scientific discussion, the results of which, as far as they go, are from time to time published in the *Government Gazette.* But no funds are available for this work, the Observatory being supposed to make and investigate only local observations, and with reference to weather-intelligence to warn the colony of storms by which it may be threatened, as far as may be possible through local observations. Some distinguished individuals having the welfare of the colony at heart would gladly see the little Observatory extended into a Meteorological Office for the Far East, for which it would be so peculiarly adapted owing to its central position, extensive telegraphic connections, &c.; but where the money is to come from has not yet been suggested. The Meteorological Office in London is allowed over ninety thousand dollars a year. The area in question is considerably larger than the area covered by the United Kingdom. The annual cost of the local Observatory was estimated to begin at ten thousand dollars, and it was remarked that additional clerical help would certainly be needed if it were resolved to undertake a thorough investigation of the monsoons of the China Sea. But actually only about six thousand dollars a year are expended in connection with the Hong Kong Observatory.

The Colony itself is warned by means of the *typhoon gun*, placed at the foot of the mast for hoisting signals beside the time-ball tower. It is fired one round whenever a strong gale of wind is expected here, and two rounds whenever the wind is expected to blow with typhoon force. It will be fired again, if possible, when the wind is likely to suddenly shift round. In 1885 it was fired also as a mail gun, but this practice has been discontinued, and as long as the typhoon gun is not fired in future, one may be sure that no typhoon is expected here. During the approach of a typhoon, and at other times when it appears desirable, special messages are telegraphed from the Observatory to be distributed in Hong Kong in such manner as the Government may from time to time see fit to direct, but as soon as they are issued from here the writer's responsibility in the matter ceases. This arrangement will, however, be found to be of very little use until the Observatory is placed in direct communication with the telegraph offices in Hong Kong, as the connections between the police stations generally break down in bad weather, when there is no boat-communication with the other side of the harbour, and thus the colony may expect that communication with the Observatory will sometimes be interrupted just at such times when the intelligence issued from here would be particularly useful. As soon as direct communication with the telegraph offices is established, the daily returns from the Treaty ports will be telegraphed across the harbour, and the *China Coast Meteorological Register* can then be issued at an early hour, by which its utility will be very much increased.

In the course of the summer of 1884 the writer invented and started a system of *meteorological signals*, which continue to be hoisted on the mast beside the time-ball tower at Tsim-sha-tsui. As these signals could not be hoisted without friendly co-operation with the officials of foreign Governments, they are, of course, unofficial, using this word in the sense in which it is understood by scientific men. The utility of these signals is confined to the shipping and to those interested in ships about to leave the harbour, or out in the China Sea. *The colony itself is warned by means of the typhoon gun.*

A red drum is hoisted to indicated the existence of a typhoon felt in the China Sea in a longitude more easterly than the colony. Steamers, if bound for northern, western, or southern ports, should lose no time in starting, and may then expect more or less fine weather. Those bound for the Philippines should take precautions to avoid the typhoon, and observe the rules set forth in the first section of this article. Sailing-vessels bound for western or southern ports should lose no time in starting, but those that are bound for northern or eastern ports ought to remain in the harbour awaiting further information, as they may expect to fall in with calms or contrary breezes after starting, even should the wind be westerly here at the time. The day after the drum has been hoisted the *China Coast Meteorological Register* should be consulted, taking into account that typhoons east and south-east of Hong Kong generally travel at the rate of from six to fourteen miles an hour.

A red cone pointing upwards indicates that a typhoon exists in a latitude more northern than the colony, or that it is progressing towards the north. More or less persistent south-west winds, at times accompanied by thunderstorms, may then be expected, and ships leaving the harbour are not at all likely to run any risk from the typhoon. Sailing-vessels bound for the north should start as soon as convenient, so as to benefit by the favourable breeze to run through the Formosa Channel and avoid the way round Formosa. By following the latter route a sailing-vessel, moreover, runs the risk of encountering the next typhoon east of Formosa, particularly during the months of August and September.

A red cone pointing downwards indicates that a typhoon exists in a latitude more southern than the colony, or that it is progressing towards the south. As such a typhoon is likely to travel in a northerly direction, ships desirous of avoiding bad weather should await further instructions, or remain in port till the barometer begins to rise. Then the danger is past.

A red ball indicates that the typhoon exists in a longitude more westerly than the colony. Ships starting for northern, eastern, or southern ports may expect breezes from east round by south to south-west. Those starting for western ports run no risk as long as the barometer is rising. If it should happen to fall, they may heave-to, and subsequently, if necessary, take refuge in some typhoon anchorage, such as St. John's harbour, but this will rarely occur. If a vessel in the Formosa Channel experiences an increasing south-westerly gale and a falling barometer, the typhoon has very likely recurved. All you have to do in that case is to lie-to, when the weather will quickly improve, and you may expect a pleasant voyage.

V. As the typhoons during their entire course are nearly always moving northwards, or rather into a higher latitude, a ship situated in the southern semicircle is on the whole in a safer position than north of the centre. East of Luzon typhoons move west-north-westward, or thereabout, and a ship must shape its course so as to reach the quadrant south-east of the centre. As a general rule, they move north-westward in that part of the China Sea between Hong Kong, Luzon, and Formosa, and east of the latter island they generally travel in some direction between north-west and north. So your vessel is safest when south of the centre, where you must heave-to till the weather improves, particularly if bound for one of the northern ports. If bound for the south, you may run across the path in front of the centre with the north-westerly breeze, but if you are not in time you may lose your boats and sustain other damage.

About 30° latitude, between China and Japan, you are liable to fall in with a typhoon travelling in any direction between west-north-west, north, and east-north-east. Here you are as a rule safest when south of the centre, but if the typhoon is travelling north-eastward this is in the dangerous semicircle. However, the investigations of the writer, though he has paid less attention to typhoons near Japan than elsewhere, nevertheless indicate with some degree of probability that the wind is less incurving behind the centre in that locality than elsewhere. North of this latitude you would of course prefer to be west of the centre.

Suppose that after leaving Singapore bound for Hong Kong the south-west monsoon begins to blow fresher and the barometer to fall, and you suspect that a typhoon is raging in a latitude more northern than where you are at present (the phenomena mentioned would not necessarily indicate the existence of a typhoon, if they were not accompanied with some of the other signs enumerated in the first section of this article), you will, of course, set your course to the east in order to sail round the typhoon and benefit by the south-easterly backing to east winds which you may expect to fall in with; but if the season is late in the year, you had better assure yourself that the typhoon is not travelling south-westward, in which case you might possibly be overtaken by the centre. These typhoons are often the cause of high seas even in the Gulf of Siam; but as their progressive motion is usually rather slow, you may heave-to in order to make observations without losing ground perceptibly. Supposing a typhoon in the China Sea does not make itself felt till you have reached a higher latitude, and that it is passing westward in a latitude south of your ship; being in the dangerous semicircle, where the wind is, moreover, stronger than south of the centre, you may have to cross the path in front of the centre to arrive in the anterior left-hand quadrant; or, if the typhoon is yet distant, the wind light, and your ship thoroughly sea-worthy and in good trim, you may possibly put her on the port tack and run north-eastward, but be ready to change the tack as soon as it becomes advisable.

Many of the anchorages along the south-eastern coast of China and the south-western coast of Formosa afford excellent shelter against north-east winds, but would prove to be much worse than the open sea during a heavy southerly gale. If you observe a northerly gale and a falling barometer, by far the surest signs of an approaching typhoon, and appearances are rapidly getting worse, then occasions may possibly occur when you may be under the dire necessity of running southwards with the northerly gale, against the rules laid down by meteorologists, and bring your ship into a most dangerous position in front of the centre. But you may happen to be better off there after gaining ample sea-room, than in the snug anchorage, where your vessel would be smashed against the rocks as soon as she began to drag her anchors when the storm burst upon her from the south, although the south-west storms experienced along the south eastern coast of China during a typhoon that enters the mainland are as a rule less violent or protracted than the preceding storm from the north.

Suppose that after leaving Hong Kong bound for a northern port you were to ascertain the existence of a typhoon about to cross your course in front of your vessel, and you experience, say, a strong breeze from the west-north-west. If you do not alter your course, you may, from the fact of its subsequently appearing to be a hanging gale and seeing the mercury falling in the barometer, draw the erroneous conclusion that you are on the path of the centre of a typhoon coming down on you from the east-north-east. In such a case it does not appear to be advisable to scud before the wind, it being decidedly wiser to heave-to. Then if the gale is observed to begin to back towards south-west, you may run southwards and shape your course so as to sail round the typhoon. Masters of steamers leaving Hong Kong while the red drum is hoisted generally lose no time in running southwards as soon as the typhoon is observed to have taken a north-western course, and suffer very little delay in consequence.

Steamers bound for Shanghai are, while between Foochow and Ningpo, liable to experience the northerly gales that precede a typhoon of the second class travelling north-westward and about to strike the coast in that locality. Not wishing to expose their ships to the high cross seas round northern Formosa, the masters generally take them into the nearest typhoon harbour in order to wait till the centre has entered the mainland, and then run northwards with the southerly gale.

These few examples will be sufficient, the more so as the further consideration of the subject would lead into details with which the writer is not familiar, being possessed of no further knowledge of navigation than the little that can be gleaned from the inspection of log-books and from occasional conversations with masters of vessels of many years' standing. The writer has invariably found these gentlemen ready to recount their experiences and to communicate any information, as soon as they found that it was required for scientific purposes exclusively. The master of a vessel, after encountering a severe typhoon, has often to undergo the vexation of seeing every manœuvre of his subjected to the comments of those unaware of the hundreds of things he has to take into consideration besides the law of storms, and who were comfortably ensconced in their houses while he was experiencing the typhoon with its fierce gusts, interrupted by the, if possible, more ominous lulls, during which he cannot see three ship's lengths before him, the mountainous waves in which his good ship is but a "cock-boat," the loudest shouting inaudible, drowned in the roar of the tempest, boats and everything movable having been washed overboard, rudder gone, and perhaps one of the masts thumping at her bottom, while the seas threaten at every moment to swamp the ship.

VI. The origin of a typhoon is not quite understood, but appears to be connected with an abnormally high temperature and humidity in some place in comparison with the neighbourhood. Over such a place the hot air expands, ascends, and is thereby cooled. But the heat liberated by the consequent condensation of water-vapour retards the rate at which it cools on rising in the atmosphere, and enables it to rise still further. When the air has risen to a high level, it effects there an increase of barometric pressure, in consequence of which the upper air is set in motion out towards the circumference of the area in question. Thus a fall in the barometer at the surface of the sea in the middle of the hot and damp region is effected, and the surrounding air is impelled in towards the centre. The motions inwards at the surface of the sea, and outwards at a high atmospheric level, are, of course, contemporaneous, and one is assisted by the other. But air in motion is deflected towards the right in the northern hemisphere, owing to the rotation of the earth, except at or very near the equator; whence typhoons do not exist in that locality, for if the wind continues to blow into the depression it is soon filled up. Owing to its deflection towards the right, the wind is caused to move in a curved path in towards the centre, and the centrifugal force, developed in the curvilinear motion, deflects it still further from the straight line leading into the centre. The friction between the wind and the greatly disturbed sea-surface likewise retards the entrance of the air at the sea-surface into the central parts of the depression. But the air at a higher level in the atmosphere is subject to little friction, and escapes therefore at a greater speed from the central high-pressure area at that level. It is, therefore, apparent that once a cyclonal motion is started under circumstances favourable for its continuation, it tends to increase and to spread outwards.

There is, however, this important difference between a typhoon and a tornado, that the latter is taller than it is broad, whereas a typhoon perhaps does not reach above an altitude of four miles, while its horizontal diameter may amount to upwards of a thousand miles. Moreover, it is not at all likely that the centre at a higher level lies vertically above the central calm at the earth's surface, or even that the centres at different altitudes are situated in a straight line. We are, therefore, scarcely entitled to speak of an axis in a typhoon.

The spirals described by the air particles approaching the centre in a typhoon are known as logarithmic spirals, but unless a typhoon is stationary, which is perhaps never the case in Nature, new portions of air are constantly set in motion in front of the centre and others left behind by the typhoon. .

As already remarked, the progressive motion of typhoons is evidently caused by the wind prevailing, if not at the surface of the earth, at any rate at a higher level. That the principal part of the disturbance is situated high above the surface of the earth is proved by the fact that the centres of typhoons pass across mountains several thousand feet high, and also by the circumstance that the difference between the temperature at the Hong Kong Observatory and at Victoria Peak is not perceptibly affected by the approach of a typhoon, for we cannot very well assume that the temperature of a vertical column of air is lower near the centre than outside the cyclone. The mountains referred to are situated on islands, and while crossing them the typhoon derives its store of water-vapour from the surrounding sea, for as soon as the centre has entered the coast, and is on all sides surrounded by dry land, it ceases to exist as a tropical storm, and can only be traced in the registers through a slight fall in the barometer, a freshening of the wind, perhaps amounting to a moderate gale at stations crossed by the centre, and wet weather. Inland in China the bull's eye of a typhoon does not appear to be observed.

As the wind blows more straight into the centre the nearer the equator you are, it follows that more air enters the typhoon from the southern semicircle than from the northern, and this is one of the reasons why typhoons nearly always move in a northerly direction. Moreover, the difference increases together with the dimension of the typhoons, which explains why they expand and accelerate their progressive motion at the same time.

The foregoing observations contain the principal practical results of investigations of about forty typhoons, continued during a period of three years. The mariner into whose hands this article may fall is advised to determine for himself the direction in which the centre of a typhoon, which he is experiencing, is travelling; for although typhoons of the classes enumerated are by far the most common, he never can be quite sure that he has not to do with an exceptional case, and quite possibly a case that is not found among the forty typhoons referred to above. By the time that we shall be in possession of full and trustworthy investigations of a couple of hundred typhoons, we may expect to have complete lists of the sub-classes of the four classes of typhoons, and to be better acquainted with cases of rare occurrence, for, after all, the typhoons are of a simpler construction and their paths more regular than is the case with storms in Europe. Typhoons are so violent near their centre that the whole disturbance is evidently ruled thereby, whereas storms in the North Atlantic and in Europe appear to be made up of a lot of local eddies, some of which are by degrees detached from the chief disturbance and form subsidiary depressions. The writer has not been able to ascertain the existence of a subsidiary depression in the China Seas during the last three years, and it is therefore doubtful whether such ever occur.

A great advance in our knowledge of typhoons in the China Sea will no doubt follow on the construction of a lighthouse on the dangerous Pratas Shoal, such as has for many years been talked about. Our storm-warnings would gain still more in value, and the cost of construction need not exceed the loss caused by a single disastrous typhoon.

EARTHQUAKE IN SIERRA LEONE

THE following correspondence has been forwarded for publication by Mr. R. H. Scott, F.R.S., Secretary, Meteorological Office:—

Government House, Sierra Leone, October 29, 1886

SIR,—I have the honour to transmit a copy of a communication received from Mr. J. M. Metzger, Manager of the Western District, in which he reports that an earthquake was felt at Sennehoo, in the Bompeh River, about the middle of last month.

(2) In the third and fourth paragraphs of his letter, Mr. Metzger draws attention to the fact that the shock in question was almost simultaneous with those experienced in other quarters of the globe, and that the latitude of the Bompeh District is within a few degrees of Charleston, America, where their effects lately proved so disastrous.

I have, &c.,
(Signed) J. S. HAY,
Administrator-in-Chief

The Right Hon. Edward Stanhope, M.P.,
&c., &c., &c.

I HAVE the honour to state, for the information of His Excellency the Administrator-in-Chief, that on the return of the District boat from the Bompeh River on the 16th inst., the coxswain reported that he had been informed at Sennehoo that about the middle of last month an earthquake was felt at that place and in the upper parts of the country; in consequence of which, many of the natives, who interpreted the event as prognostic of coming war, hastened down to the water-side to procure arms and powder in preparation for hostilities, which they regarded as imminent.

(2) The shock is said to have been continuous, accompanied with a rumbling noise as of some heavy-laden cart being moved along, resulting in the cracking and falling down of the mud plaster on the walls of the houses at Sennehoo. What happened in the upper parts of the country is, of course, not known, but the force must have been sufficiently severe to impress the people and influence them as they appeared to have been.

(3) It is remarkable that these vibrations, which seemed to have been extensive throughout the Bompeh District, and which seemed to have been so distinct, are almost simultaneous with those experienced in some places in the Mediterranean Sea, in Greece, and notably at Charleston, on the Atlantic coast of America, where their effects were so disastrous.

(4) The Bompeh, like the Ribbee and Cockborough Rivers, runs into Yawry Bay, which is an arm of the Atlantic, and the Bompeh District, on the eastern side of this ocean, is opposite to, and not many degrees of latitude below, the scene of the late disasters in America.

(5) I think it my duty to make this communication, as the information might possibly be of use to scientists engaged in the study of the facts connected with the range and transmission of these seismic disturbances.

(Signed) JOS. M. METZGER, Manager
Kent, Western District, October 20, 1886

SCIENTIFIC SERIALS

American Journal of Science, November.—The higher oxides of copper, by Thomas B. Osborne. The oxides here dealt with are copper dioxide and copper sesquioxide; but being unable to continue the subject, at least for some time, the author publishes the incomplete results so far obtained, in the hope that they may be of use to others wishing to continue this line of investigation. —The structure of the Triassic formation of the Connecticut Valley, by William Morris Davis. It is shown that disturbance has taken place after the period of deposition; that it was not caused by overflow or intrusion of trap-sheets; that it was not a simple monoclinal tilting; and that there is evidence for occurrence of unseen faults. The probable character of the disturbing force, its action on the fundamental schists, with consequent monoclinal faulting of overlying Triassic strata, and generally the area and depth of the disturbance, are questions also discussed in this elaborate paper.—Researches on the lithia micas, by F. W. Clarke. Descriptions and exhaustive analyses are given of the lepidolites of Rumford, Hebron, Auburn, and other parts of Maine, and of the iron-lithia micas of Rockfort granite-quarries near Cape Ann, Massachusetts.—The thickness of the ice in North-Eastern Pennsylvania during the Glacial epoch, by John C. Branner. So far from rising only 2200 feet above sea-level, as hitherto supposed, the ice is shown to have covered the twin peaks of Elk Mountain (2700 and 2575 feet), and no doubt also the Sugar Loaf, Ararat, and the other loftiest summits of this region during the Glacial epoch. A sheet of ice 1500 feet or less in thickness could never have flowed across such a mountainous region, so regardless as the great glacier was of its marked topographical features.—On the time of contact between the hammer and string in a piano, by Charles K. Wead. Owing to the uncertainty attending the theory developed by Helmholtz regarding the action of the hammer on a piano-string, the author has attempted to measure directly the time of contact by a simple process with results here tabulated.—Photographic determinations of stellar positions, by B. A. Gould. This is a reprint of the paper presented at the late Buffalo meeting of the American Association, and containing a brief history of stellar photography, and of the results so far obtained by the author in this department of practical astronomy.—Lucasite, a new variety of vermiculite, by Thomas M. Chatard. A description and full analysis is given of this substance, specimens of which have been found associated with corundum at Corundum Hill, Macon County, North Carolina. It has been named lucasite in honour of Dr. H. S. Lucas, so well known in connection with the Chester emery mine, Massachusetts.—Crystallographic notes, by W. G. Brown. An account is given of certain artificial copper crystals, of artificial crystallised cuprous oxide (cuprite), and of crystallised lead carbonate (cerussite) found under circumstances here described.—On the chemical composition of ralstonite, by S. L. Penfield and D. N. Harper. A comparative table is given of the analyses made by Nordenskjöld, Penfield, and Brandle of this rare mineral, which was found associated with thomsenolite at Arksuk Fjord, Greenland. —Analyses of the thomsenolite by the same chemists.—Mineralogical notes, by Edward S. Dana. Descriptive analyses are given of columbite from Standish, Maine, of diaspore from Newlin, Pennsylvania, of zincite from Stirling Hill, New Jersey, and of some native sulphur from Rabbit Hollow, Nevada, interesting because of its complex crystalline form.

Rivista Scientifico-Industriale, October.—On the cause of the magnetic rotatory polarisation, by Prof. Augusto Righi. Fresnel's hypothesis having been proved inadequate by recent experiment, the author has resumed the subject, with the view of ascertaining whether it may be explained by the reflected or transmitted vibrations of bodies endowed with rotatory power. If the incident polarised ray on penetrating a body is really decomposed into two inverse circular rays endowed with different velocities, the intensity of the two rays must also vary both in the reflected and transmitted light. The ray possessing greatest velocity of propagation, and consequently a lower index of refraction, must possess least intensity in the reflected and greatest in the transmitted light, assuming the two indices to be greater than unity, as in the opposite case the result would be reversed. Hence both the reflected and transmitted ray will become elliptical; and Prof. Righi has succeeded in determining this ellipticity by employing iron, the body endowed with the greatest rotatory power. The elliptical vibration of the reflected ray is in the opposite direction to that of the magnetising current, while that of the transmitted ray is in the same direction. In a future communication it will be shown that this agrees with the hypothesis of double circular refraction.—On the tests of fatty substances, and especially of olive oil, by Professors G. B. Bizio and L. Gabba. This paper contains a critical inquiry into the methods of testing the purity especially of olive oil, and it concludes that none of the processes now in use are absolutely trustworthy. Even that of Bechi fails to distinguish with certainty between olive and cotton oil.

SOCIETIES AND ACADEMIES

London

Royal Society, November 18.—"A Theory of Voltaic Action." By J. Brown. Communicated by Lord Rayleigh, Sec. R. S.

The difference of potential near two metals in contact is due to the chemical action of a film of condensed vapour or gas on their surfaces. Such a pair of metals is thus similar to a galvanic cell with its electrolyte divided by a diaphragm of air or other gas, and it is the difference of potential of the films that is measured in "contact" experiments; the metals themselves being at one potential.

Experiments with an electrometer having quadrants of the metals under examination, the construction of which was described, were made on the rate of decrease of the difference of potential near two metals in contact, and exposed to the action of the air and of other gases; also where a change in the constituents of the atmosphere surrounding a pair of metals in contact reverses the difference of potential near them in correspondence with the reversal of electromotive force which takes place after a similar change in the corresponding liquid electrolyte used with the same metals as a voltaic cell.

Such reversal takes place with pairs of copper-iron when hydrogen sulphide gas, or ammonia gas, is added to the air surrounding them; with silver-iron, when hydrogen sulphide is added; and with copper-nickel when either ammonia or hydrochloric acid gas is added.

Neutral or inert gases have little or no effect.

Covering the metals with varnish, or immersing them in naphtha, to protect them from atmospheric action, reduced the difference of potential near them considerably, but not to zero.

Drying the atmosphere about a copper-zinc pair by means of phosphoric anhydride in one instance reduced the difference of potential in 173 days from ·66 to ·5 Daniell. Then, on opening the instrument, it rose to ·67 Daniell.

A permanent current was produced by placing the (apparently) dry plates of copper and zinc in close proximity, so that their films were in contact. When the plates were either brought into actual metallic contact, or separated farther apart than a certain distance, as stated the current ceased. This "film-cell" could also be polarised by sending a current through it from another battery.

Modifying an old experiment, due to Gassiot, so as to avoid any contact of dissimilar metals, it was shown that, when the zinc plate of the volta condenser was joined to the zinc quadrant of the electrometer and the copper of the condenser to the copper of the electrometer, on altering the capacity of the condenser an alteration of the difference of potential near the quadrants was produced.

In an appendix, Mr. J. Larmor, of St. John's College, Cambridge, points out the difficulty of explaining this last experiment by any hypothesis other than that of some kind of chemical action at the surface of the metals.

November 25.—A paper by Sir Richard Owen, F.R.S., was read, of which the subject was a fossil lower jaw of the large extinct marsupial quadruped which the author, from previous fragmentary specimens, had referred to a carnivorous pouched species of the size of a lion, to which was assigned the generic name *Thylacoleo*, and the probable prey of which had been the larger forms of herbivorous marsupials, which, with their destroyer, had become extinct.

Several species, allied to the kangaroo, but equalling respectively in bulk a rhinoceros, an ox, an ass, had become extinct. The largest existing kind, to be seen in the Zoological Gardens, was named by Dr. Shaw, *Macropus major*. It has escaped extinction by its swiftness and power of concealment in the "scrub." Wombats, also, of the size of fallow-deer, co-existed with the huge kangaroos; the small kinds, capable of concealing themselves in burrows, alone survive.

Remains of the large extinct marsupials, both devourers and prey, are to be seen in the Geological Department of the Museum of Natural History; they are described and figured in the author's work on "Fossil Marsupialia." Their extinction is attributed, with that of the wolf-like *Cynocephalus* and the *Thylacoleo*, to the aboriginal natives of Australia.

Linnean Society, November 18.—W. Carruthers, F.R.S., President, in the chair.—Mr. H. Bury was elected a Fellow of the Society.—Mr. W. H. Beeby showed specimens of *Callitriche truncata*, Gussone, from near Westerham, Kent. The species had long been supposed to be extinct in this country, being only known as British from dried specimens from Sussex in Borrer's Herbarium.—Mr. D. Morris exhibited two enlarged photographs of the Castilloa Rubber-tree of Central America (see *Trans. Linn. Soc.* Botany, 2nd ser. vol. ii. part 9). The larger photograph illustrated the manner in which trees were treated to extract rubber, by a special cut from above downwards. Trees of ten years old and upward are said to yield about eight gallons of milk at the first bleeding. This milk is coagulated by the use of the juice of *Calonictyon speciosum*, and the rubber prepared by washing and pressing. Mr. Morris described the habit and growth of the trees in their native forests, and expressed the opinion that for cultural purposes this rubber-tree may be better suited to the circumstances of planters than any other. It could be utilised as a shade tree in cacao and coffee plantations, and yield at the end of ten years at the rate of twenty shillings per tree in marketable rubber. In British Honduras trees are tapped for rubber every three or four years.—Mr. A. D. Michael exhibited living specimens and preparations of an *Argas*, received from Mrs. Crawford, the State Entomologist of Adelaide, Australia. The insects in question appear to be identical with the much-dreaded *Argas persicus*, Fischer, the bite of which was supposed to cause madness and death.—Mr. H. N. Ridley made remarks on specimens in spirit and drawings of species of *Coryanthes*, viz. *C. macrantha*, Hook., and *C. maculata punctata*. He mentioned that Mr. Rodway, of Demerara, had lately published observations showing that the statement of Crüger, hitherto believed, as to the fertilisation by bees, did not obtain in all the species; inasmuch as in *C. speciosa* he (Mr. Rodway) had noticed that a kind of green fly was the fertiliser.—Mr. Geo. Murray exhibited specimens of *Rhipilia* in spirit from Grenada, West Indies, obtained at a depth of five fathoms.—Mr. W. Fawcett, exhibited coloured drawings of *Hydnora abyssinica* and *H. bogosensis*, sent by Signor Beccari from Florence. They clearly showed the difference between the two species, in colour, and in the former having a book-like process below the apex, and its ramentiferous surfaces have long ramenta at their margins. Both species differ from *H. africana* in the ramentiferous surfaces not extending to the apex.—Mr. C. T. Musson drew attention to a branch of a blackthorn, obtained near Newark, showing a curious malformation of the branchlets. —Dr. Maxwell Masters read a paper on the peculiar conformation of the flowers of *Cypripedium*. The explanation may be sought in the course of development, in the minute anatomy and arrangement of the fibro-vascular bundles, and in the examination of the comparative morphology of the flower; organogeny affords in this case only doubtful testimony, as the flower is irregular from the first. The distribution of the primary fibro-vascular bundles, and of the offshoots from them, affords more conclusive evidence of the true construction of the flower, and if studied in conjunction with the comparative morphology leads to very satisfactory results. By these means it becomes easy to refer the flower to the ordinary type seen in a regular pentacyclic and trimerous monocotyledon, and from which it is reasonable to infer it may have originated. The deviations from the type have arisen from concrescence or inseparation of some parts, inordinate development of others, and complete suppression of a third series. The author cited instances showing numerous intermediate gradations between the ordinary conformation of *Cypripedium* and that of the ideal type trees, proving that what was, at first, a matter of speculation and inference from imperfect evidence, was borne out by actual facts. The illustrations brought forward afforded examples of the reduction of parts and the increased number of parts, in connection with which the author alluded to the special tendency to develop the second or inner row of stamens, as happens in Restiaceæ and Xyridaceæ, while in Iridaceæ the opposite tendency is manifested. Another series of illustrations comprised cases of regular and of irregular *Peloria*, which were of special importance as affording evidence on the one hand of the probable past conformation of the flower, and on the other of the probable course of development in the future.—The fifth and concluding part of the Rev. A. Eaton's monograph of recent Ephemeridæ or may-flies was read. The author says 55 genera and 270 species have been characterised, in addition to 11 nymphs and 19 species of doubtful position; 5 genera and 68 species are new to science.—Mr. J. G. Baker read a paper entitled "Further Contributions to the Flora of Madagascar," in which upwards of 250 new plants, seven of them new genera, gathered recently by the Rev. R. Baron, F.L.S., are described. Of the new genera, one belongs to Menispermaceæ, one to Geraniaceæ near *Impatiens*, one to Rubiaceæ, and two each to Melastomaceæ and Compositæ. Of well-known Cape types, *Pelargonium*, *Stoebe*, *Cineraria*, and *Belmontia* are now for the first time added to the Madagascar flora. The faint affinity of the flora of Madagascar to that of India and Malaya is strengthened by the discovery of the genus *Cyclea*, and of new species of *Alyxia*, *Didymocarpus*, and *Strobilanthes*. Of types of economic interest there are new species of *Dalbergia*, *Macaranga*, *Strychnos*, *Balsamodendron*, and *Garcinia*. It seems that during the last ten years between 1100 and 1200 new plants from Madagascar (29 of which are new genera) have been described in the *Journal* of the Linnean Society and *Journal of Botany*, nearly all of them collected by our own countrymen.

Chemical Society, November 18.—W. Crookes, F.R.S., Vice-President, in the chair.—The following papers were read:—Researches on the relation between the molecular structure of carbon compounds and their absorption-spectra; part 8, a study of coloured substances and dyes, by W. N. Hartley, F.R.S.—Spectroscopic notes on the carbohydrates and albumenoids from grain, by W. N. Hartley, F.R.S.—Preliminary note on the electrolysis of ammonic sulphate, by Herbert McLeod, F.R.S.—The preparation and hydrolysis of hydrocyanides of the diketones, by Francis R. Japp, F.R.S., and N. H. J. Miller, Ph.D.—The action of salicylic aldehyde on sodium succinate in presence of acetic anhydride, by Gibson Dyson.—The reduction of nitrites to hydroxylamine by hydrogen sulphide, by E. Divers, F.R.S., and T. Haga.—Note on some double thiosulphates, by J. B. Cohen, Ph.D.—Preliminary note on the action of triphenylmethyl bromide on ethyl sodio-malonate, by George G. Henderson, M.A., B.Sc.—Action of silicon tetrachloride on aromatic amido-compounds, by Arthur Harden, B.Sc.

Physical Society, November 27.—Prof. W. G. Adams in the chair.—The following papers were read:—On a method of measuring the coefficient of mutual induction of two coils, by Prof. G. Carey Foster, F.R.S. The two coils are for convenience designated by P and S (primary and secondary). The method as originally devised consists of two parts: (1) observing the swing of the needle of a galvanometer placed in series with the secondary coil when a current of strength γ is started in the primary; (2) placing the galvanometer and a condenser of known capacity, c, as a shunt between two points, A and B, of the primary circuit, such that the first swing of the galvanometer needle on completing the primary is the same as in (1). It is easily seen that under these conditions $M = crr_1$, where M is the coefficient to be determined, r = resistance between the points A and B, and r_1 = resistance of galvanometer and secondary coil. The method requires the value of γ to be the same in the two experiments, and facilities for varying r without altering γ. To overcome these difficulties the arrangement has been modified so as to make it a null method. The connections remain the same as in (2), excepting that the ends of the secondary coil are connected to the terminals of the galvanometer through a variable resistance with no self-induction. If p be the resistance of the secondary coil and variable resistance when adjusted, so that, on completing the primary circuit, the integral current through the galvanometer is zero, it is shown that $M = cpr$, where c and r have the same meaning as before. For let A and E be the potentials of the galvanometer terminals at any time, t, q the resistance of galvanometer, y the current through it, and N and L the coefficients of self-induction of the galvanometer and secondary coil respectively. Then considering the path from A to E through the secondary coil we have—

$$A - E = px + L\frac{dx}{dt} - M\frac{d\gamma}{dt}.$$

For path through galvanometer—

$$A - E = qy + N\frac{dy}{dt}.$$

Equating these, and integrating from $t = 0$ to $t = \infty$, we get—

$$p\int_0^\infty xdt - M\gamma = 0.$$

Since $\int_0^\infty xdt$ = charge of condenser,

$$= c\gamma r,$$

we see that $$M = cpr.$$

It is easily shown that if $L = M$ then $A - E = 0$ for all values of t. Hence the galvanometer might in this case be replaced by a telephone. By inversion, the method could be used for determining the capacity of condensers in absolute measure if M be known. The author thinks the method will be useful for dynamo-machines, and gave a series of numbers obtained by experiments on different coils, showing that it gives consistent results. Mr. C. V. Boys suggested that, by arranging a switch to change the connections from (1) to (2) in rapid succession, a steady deflection might be obtained, and thought that this would enable very small coefficients to be determined. Remarks by Prof. Forbes, Prof. Adams, and Prof. Perry were answered by Prof. Foster and Dr. Fison.—On the critical mean curvature of liquid surfaces of revolution, by Prof. A. W. Rücker, M.A., F.R.S. The paper is chiefly mathematical, and deals with liquid surfaces of revolution attached to two circular rings, the planes of which are at right angles to the line joining their centres. By "mean curvature" the author designates *half the sum of the reciprocals of the two principal radii of curvature of the surfaces*. Maxwell has shown in his article on "Capillary Action" ("Encyclopædia Britannica"), that, if the film be a cylinder, a slight bulge will cause an increase or decrease in the mean curvature according as the length is $<$ or $> \frac{\pi}{2}$ times the diameter. If $l = \frac{\pi}{2} d$, then a small change in the volume of the surface will modify its form, but will not alter the mean curvature. Thus the mean curvature of such a cylinder is evidently a maximum or minimum with respect to that of other surfaces of constant mean curvature, which pass through the same two rings at the same distance apart, and which differ but little from the cylindrical form. Hence the cylinder may be said to have a *critical mean curvature* when the distance between the rings is $\frac{\pi}{2}$ times their diameter. If the distance between the rings is altered, a similar property is possessed by some other surface. The author's paper investigates the general relation between the magnitude and distance apart of the rings, and the form of the surfaces of critical curvature. If x is the axis of revolution, then the equation to a liquid surface of revolution is given by the expressions—

$$x = \alpha E + \beta F, \qquad y^2 = \alpha^2 \cos^2\phi + \beta^2 \sin^2\phi,$$

where F and E are elliptic integrals of the first and second kinds respectively, of which the amplitude is ϕ, and the modulus $K = \sqrt{\alpha^2 - \beta^2}/\alpha$ as usual, $\Delta = \sqrt{1 - K^2 \sin^2\phi}$, whence $y = \alpha\Delta$, and if $K = \sin\theta$, then $\beta = \alpha \cos\theta$, and since $\alpha > \beta$, α and β are the maximum and minimum ordinates. The results show that as θ increases from 0° to 90°, the surface of critical mean curvature is an unduloid with limits of cylinder and sphere, when $\theta = 0°$, and $\theta = 90°$ respectively. When θ passes from 90° to 180°, the surface is a nodoid with limits of sphere and a circle whose plane is perpendicular to the surface of revolution. In the third quadrant the surface is still a nodoid the limits of which are a circle and the catenoid. Finally, in the fourth quadrant the surface is an unduloid, the limits being the catenoid and cylinder. Experiments were shown proving that with cylindrical films, where $l < \frac{\pi}{2} d$, increase of internal pressure produced a bulging, whereas if $l > \frac{\pi}{2} d$, a bulging was produced by decrease of pressure. From this it is evident that if the interiors of two cylindrical films, whose $l < \frac{\pi}{2} d$, be connected, stable equilibrium will result, whereas if $l > \frac{\pi}{2} d$, there will be unstable equilibrium. These facts were illustrated experimentally with great success. After some remarks by Mr. C. V. Boys, the proceedings terminated.

Anthropological Institute, November 23.—Francis Galton, F.R.S., President, in the chair.—The election of Mr. C. W. Rosset as a Corresponding Member was announced.—Prof. A. H. Keane read a paper, by Consul Donald A. Cameron, on the tribes of the Eastern Soudan.—The Assistant Secretary exhibited, on behalf of Mr. J. Olonba Payne, nine symbolic letters (Aroko) as used by the tribe of Jebu in West Africa.—The Secretary read a paper, by Mr. T. R. Griffith, on the races inhabiting Sierra Leone.—The Rev. George Brown gave a brief *résumé* of his paper on the Papuans and Polynesians, the reading of which was adjourned till the next meeting.

PARIS

Academy of Sciences, November 29.—M. Daubrée in the chair.—The medal prepared by the youth of France to commemorate the centenary of M. Chevreul, was presented to the President of the Academy, with some appropriate remarks by M. de Quatrefages. The medal, which is a fine work of art by M. Roty, bears on one side a bust of the illustrious *savant*, and on the reverse his full figure, seated in an armchair, in an attitude of study, with the legend "La Jeunesse française au Doyen des Étudiants, 31 Août, 1786—31 Août, 1886." After defraying the expenses of this and another medal of smaller size, a copy of which will be supplied to all subscribers, the Committee has a large balance in hand, which it proposes to utilise by issuing a complete *Catalogue raisonné* of M. Chevreul's works.—Remarks on the 210th volume of the *Connaissance des Temps* (for 1888), presented to the Academy by M. Faye.—A contribution to the history of the decomposition of the amides by water and the diluted acids, by MM. Berthelot and André. These studies have been undertaken for the purpose of better determining the degree of stability, in presence of the acids, of some typical amides, which play an essential part in the tissues of organised beings, such as urea, asparagine, and others.—Glycose, glycogene, and glycogeny, in relation to the production of heat and of mechanical power in the animal system, by M. A. Chauveau. In this second communication the author deals with combustion and the development of heat in the organs while at work. In this state of physical activity the quantity of glycose which disappears in the capillaries is increased, and is in proportion with the excessive activity of the combustions excited by the play of the organs.—Fluorescences of manganese and bismuth, by M. Lecoq de Boisbaudran. In this preliminary paper three conditions are considered: (1) a single solid dissolvent and two active substances, each fluorescing with this dissolvent; (2) a solid dissolvent and two active substances, of which one alone fluoresces with the dissolvent; (3) two solid dissolvents and one active substance fluorescing with each of the dissolvents.—Treatment of the grape-vine with the salts of copper against mildew, by MM. Crolas and Raulin. Quantitative analyses are given of the amount of copper detected in the products of vines treated by this process. Although the actual quantity is never really dangerous, special precautions are recommended in all cases where the grape is intended for consumption.—On the phosphates and arseniates of silver, by M. A. Joly. The precipitated triargentic phosphate, PO_8Ag_3, obtained by double decomposition, is shown to be amorphous, easily dissolving in phosphoric acid even at a low temperature. The limits are determined within which the concentration of the phosphoric solution should be varied in order to obtain at pleasure the crystallised triargentic phosphate or the diargentic phosphate, PO_8Ag_2H.—On some coloured reactions of the titanic, niobic, tantalic, and stannic acids, by M. Lucien Lévy. Some new specific characters of these acids are described, the reagents employed being substances which nearly always present a phenolic function. Hence reciprocally these latter may in their turn be characterised by the same mineral acids.—On the conditions favourable to the restoration of the elements of the transparent cornea, by M. Gillet de Grandmont. These conditions are shown to be suppression of the suppuration, absolute repose, and absence of all intervening irritation.—On a process for intensifying the normal virulence of the microbe of symptomatic charbon, and restitution of the original activity after attenuation, by MM. Arloing and Cornevin.—Note on some essays in antituberculous vaccination, by M. Vittorio Cavagnis. These experiments were made according to M. Pasteur's method, on some rabbits and guinea-pigs, with but partial success. The author is now endeavouring to ascertain whether this method of vaccination is at all applicable to tuberculosis.—On the conformation of the external reproductive organs in the female of the anthropoid apes of the genus Troglodytes, by M. A. T. de Rochebrune.—Observations on the continuous blastogenesis of *Botrylloides rubrum* (Milne-Edwards), by M. S. Jourdain.—New methods of preparing the crystallised carbonates, by M. L. Bourgeois.—The Uralian Society of Naturalists informs the Academy that it is organising at Ekaterinburg, Russia, a Scientific and Industrial Exhibition for Siberia and the Ural Mountains, which will be opened on May 15/27, 1887, and closed on September 15/27 following.

STOCKHOLM

Academy of Sciences, November 10.—On a recently discovered map of Scandinavia and parts of the North Atlantic, edited, in 1539, by Olaus Magnus, a Swedish Catholic clergyman, by Baron A. E. Nordenskjöld.—On the mineral thorite, from two new localities, by the same.—On the Quaternary strata of the Isle of Gothland, by Herr H. Munthe.—On the development of bi-periodic functions in the series of Fourier, by Dr. C. Charlier.—Contributions to the knowledge of the infloration and fructification of the Scandinavian alpine plants, by Dr. C. Lindman.

BOOKS AND PAMPHLETS RECEIVED

Histoire Générale des Races Humaines: A. de Quatrefages (A. Hennuyer. Paris).—Microscopic Fungi. 5th edition: Dr. M. C. Cooke (Allen).—The Greyhound: H. Dalziel (Gill).—British Dogs: H. Dalziel (Gill).—British Cage-Birds: R. L. Wallace (Gill).—On the Conversion of Heat into Work: W. Anderson (Whittaker).—Lives of the Electricians: W. T. Jeans (Whittaker).—Clark's Transit Tables for 1887: L. Clark (Spon).—A Text-Book of Steam and Steam-Engines: Prof. A. Jamieson (Griffin).—Outlines of Quantitative Analysis: Prof. A. H. Seaton (Griffin).—Studien über Protoplasmamechanik: Dr. G. Berthold (Felix, Leipzig).—Proceedings of the Queensland Branch of the Geological Society of Australasia, vol. i. (Watson, Brisbane).—A New Department in Science: Dr. C. Radcliffe (Macmillan).—The Mystery of God, 2nd edition: T. V. Tymms (Stock).

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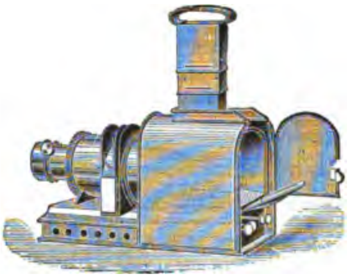

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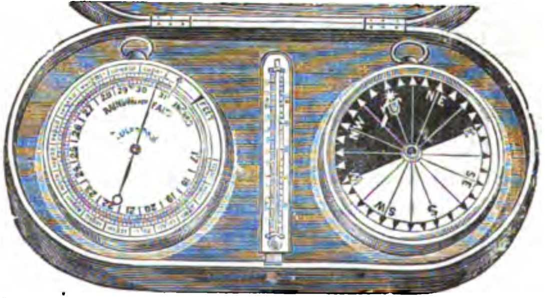

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THURSDAY, DECEMBER 30, 1886

BABINGTON'S "BIRDS OF SUFFOLK"

Catalogue of the Birds of Suffolk; with an Introduction and Remarks on their Distribution. By Churchill Babington, D.D., &c. Reprinted from the *Proceedings* of the Suffolk Institute of Archæology and Natural History. 8vo, pp. 281. Map and 7 Plates. (London: Van Voorst, 1884-86.)

NUMEROUS as have lately been contributions to local British ornithology, the treatment of the subject is very far from being exhausted, and Dr. Babington's book is extremely welcome as supplying a new catalogue of the birds of a county having so favourable a situation as Suffolk. For though wanting the extended sea-board of one neighbour, Norfolk, which meets the uninterrupted roll of the polar waves, and possessing an almost even coast-line, very unlike the irregular contour of its other neighbour, Essex, Suffolk yet contains the most easterly point of England in Lowestoft Ness, as it is still fondly called, though a "ness" is there as hard to recognise in these days as is the "bay" of its historic Solebay, a few miles further south. Suffolk also is not without its "broads"—at Fritton, Oulton, Benacre, and Easton—insignificant as they may be in comparison with those of the northern half of the ancient East Anglian kingdom. It also shares with Norfolk the great Breydon Water, and with Essex the wide mouth of the Stour, while it has for its own the estuaries of the Blythe, the Alde, the Debden, and the Orwell, by no means despicable, even if they are not equal in size to those of the Colne, the Blackwater, and the Crouch, that drain so much of Essex. Suffolk again has a natural feature, the like of which is not possessed by either of its neighbours:—

> "On Orfordness lies many a stone,
> But Dungeness has ten for one,"

says the old adage, and it is not until the south-eastern corner of Kent is reached that a similar "beach" is presented, and that one only to be surpassed by the Chesil Bank of Dorset. Highly cultivated, too, as now is almost every acre in Suffolk that will repay cultivation, there are still some wide tracts along its eastern border, and again towards its north-western extremity, which, if indeed they have ever been under the plough, have long since lapsed into an approach to their original condition, and are overgrown with heather and gorse, or form "brecks," kept, by the teeth of countless sheep and rabbits, in the state of the poet's "smooth-shaven green." The western limits of the county not only bound, but slope into, the great Fen district, that spreads for miles and miles in an almost level plain towards the Wash. The chief part of Suffolk has long been inclosed, presenting, in the absence of any but the most inconsiderable elevations, a very uniform appearance; and, were it not for its numerous woods—not many of which are really ancient—and plantations, would afford harbour to few but the commonest of birds. An indefinite district, the soil of which is of the stiffest clay, is colloquially named "High Suffolk"; but where it begins or ends, no one knows; and, for some mysterious reason, nobody will own to living in it. "High Suffolk" always begins in the next parish, or the next parish but one! A great contrast to these heavy lands is presented by the "breck" district already mentioned, where the chalk-formation comes nearly to the surface, and is only overlain by a few inches of the lightest sand—so light, indeed, that some places may be found as bare of vegetation as is a real desert—every particle of fertilising matter having been blown away by the wind after a spell of dry weather; and it will be remembered that in East Anglia the rainfall is less than in any other part of England. This district still retains, in at least one of its birds, in some of its insects, and in a few of its plants, indications of having been once—and that perhaps not so very long ago—a littoral, an arm of the sea having doubtless reached its low hills, and in after times retreating, having left these survivors who still hold their ground. But here we may say that we cannot for a moment subscribe to the opinion to which Dr. Babington gives currency (p. 123), though not saying whether he himself shares it, that the marine connexion was by "a broad estuary running from the South Suffolk coast between Bury St. Edmund's and Stowmarket through Thetford." So far as we are aware, there is no evidence in favour of such a violent supposition, and much against it. On the other hand, a very slight depression of the surface would once more bring the sea from the Wash up to Brandon, if not to Thetford.

We make no attempt to trace the deeper effects of geological formations and changes; but all these superficial characters, here so briefly sketched, combined with the geographical situation of the county, will serve to show why Suffolk should present a field of great interest to the ornithologist; its varied features offering suitable accommodation for many kinds of birds of diverse habits, and its eastward position a sanctuary where the wings of many a weary wanderer from afar may be folded at rest. There is the more need to urge the importance of these favourable circumstances, because they cannot be said to be too prominently laid before his readers by Dr. Babington, who perhaps through modesty, or perhaps through prudence (in which latter case he is certainly to be commended), abstains from setting forth the advantageous conditions of existence that the county of his adoption thus affords, albeit he devotes a few pages (258-268), which might well have been more, to the subject.

In computing the birds of a circumscribed area, it is always a difficult task to decide whether the adventitious strangers whom the accidents of travel may have driven upon its coast should be enumerated among its real inhabitants, for there is really much to be said on both sides of the question. At first sight it seems most absurd that, granting even there is no reasonable probability of its importation, the stray example of an exotic species, whose home may perhaps be in the further wastes of Northern Asia or the wilds of Arctic America, should be enrolled as a "British bird," because it has had the ill-luck to find its way hither and be killed—*secundum usum Anglicanum*—within the confines of the United Kingdom; but almost immemorial practice may be pleaded for this view of the case, and we are not minded to place on record a distinct decision against the claims of a local faunist on so delicate a question. However, the local faunist should recognise the fact that a long list is not necessarily "a strong list"—to use Dr. Babington's expression—and if space allowed us to go into details we should be inclined to strike off not a few species from his register. It is true that this would not materially alter his position, for a corresponding number would on the same ground have to be struck off the register of other counties. In reality, no one has ever doubted that the Suffolk roll is one of the highest to be found in England. Perhaps it would stand only second to that of Norfolk on the English record, for though both, so far as published lists go, are inferior to that of Yorkshire, we are persuaded that this last has been unduly swollen. We have a strong suspicion that a Kentish list would run any of them very hard; but we here speak without facts, for ornithologists have long been scarce in Kent, and no attempt at a Kentish list has been made for many a year. The comparison instituted by Dr. Babington between the ornithological wealth of Suffolk and certain other counties is in some measure fallacious,—the last list of Sussex birds, for example, dates from 1855 (not 1865 as he inadvertently states), while practically it was compiled in 1849, since which time a good many things have happened. Comparison with inland counties is of course misleading, and probably the well-known published catalogues for Cornwall, Somerset, Northumberland, and Durham, and for the Humber district, are alone those with which catalogues for Suffolk and Norfolk can be rightly compared; while the county last named, from the abundance of ornithological observers it has produced, is manifestly favoured in the race. One other thing may perhaps be mentioned in this connection, and that not so much for Dr. Babington as for authors of future "Avifaunas"; the ornithological richness of a district depends far more on the number of its real inhabitants than on the number of species which have occurred as stray visitors within its limits and only *boná fide* travellers. As regards large areas this is a truth so obvious that our remark may seem to be a platitude, but as regards small areas the consideration is too often overlooked.

Among all the English works on local ornithology with which we are acquainted, Dr. Babington's holds a peculiar place. Its contents are distinctly matters of fact, or of what passes for fact; in other words, it is a summary of records. No one would pretend to say that any book of this kind is, or could be, exhaustive; but the author has done his best to make his work so, and the infinite pains he has taken to be precise are present on every page—for every page bristles with references that have obviously cost him immense labour to collect, and his patient industry in culling them deserves the highest praise. On the other hand, this very precision may not unfrequently mislead the unwary. Unless the reader have a competent knowledge, elsewhere obtained, he may be apt to presume that the fact of such or such a species having been recorded as occurring or breeding at such or such a place and at such or such a time is an indication that it has not occurred or bred there at any other time. For the sake of those who are beginners, or ill-instructed in ornithology, and they ought to form a majority of those who use this book, it would have been better had the author uttered a warning against this kind of misconception, which in many cases is certain to follow from this concise method of citing previously recorded observations. Experts, of course, will not be taken in by it, but we think it may deceive others. Experts, however, unless they be accustomed to the way in which local floras are compiled, have some right to complain of the application of botanical methods to a fauna—for it is plain that the "Catalogue of the Birds of Suffolk" is planned on essentially the same principle as would have been a catalogue of the plants of the same county, and not according to any zoological precedent.

A few words are Dr. Babington's due on another matter. To most zoologists his name will be new, and yet he entered the field of biological literature nearly five-and-forty years ago! His ornithological appendix to Potter's "History and Antiquities of Charnwood Forest," published in 1842, was a respectable, not to say ambitious, performance for an undergraduate; and, while showing rudiments of the same scrupulosity as is seen in the present work, is equally removed from loquacity, though containing some information that the British ornithologist would not willingly let die. Both in conception and in execution it naturally has been surpassed by later publications, nor can it be regarded as the original precursor of the numerous local "Avifaunas" of Britain The primacy in this respect[1] belongs, we believe, to one the author of which has lately died, and to his memory we take this occasion of offering a passing tribute. The "Ornithological Rambles in Sussex," to which was added a catalogue of the birds of that county, appeared in 1849, the work of Mr. Arthur Edward Knox, who died on September 23, 1886, having nearly completed his seventy-eighth year. Mention of this observant naturalist, agreeable author, and accomplished gentleman is all the more needed, since his death obtained scant, if any, notice in the newspapers of the day, though column after column in their broad sheets chronicled the career of a successful horse-jockey who expired not long after. Mr. Knox, it is true, never assumed the character of a man of science any more than that of a man of letters, yet his literary style was of the best, while few professed naturalists more thoroughly practised scientific methods of observation, and none could more fully appreciate scientific worth. His three works—that already named, his "Game-Birds and Wild Fowl," and his "Autumns on the Spey"—all of the kind that is usually called "popular," have some characteristics that at once distinguish them from so many others to which that epithet is commonly applied. They are always accurate, seldom trivial, and never vulgar.

To return, however, to Dr. Babington's little volume. Its value, notwithstanding some shortcomings to which we have referred, is great, and the recorded facts, with which, as already stated, it is crammed, are such as no "British" ornithologist can afford to neglect. As a final mark of attention, let us notice that Dr. Babington's scholarly instinct has inspired him with enough courage to be the first writer who has corrected an unhappy mistake made by Linnæus, and restored (pp. 200-203) the old spelling *Podicipes*, for the ungrammatical, senseless, and misleading *Podiceps*, thereby removing a reproach which every literary man could successfully cast at a zoologist. ***Exemplum sequendum!***

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[1] Of course there are several other local lists of older date, from that of Markwick downwards, including "The Norfolk and Suffolk Birds" of Sheppard and Whitear; but these were published in journals (mostly in the Linnean *Transactions*), and we are here speaking of separate works the scope of which is ornithology alone.

INTERMITTENT DOWNWARD FILTRATION

Ten Years' Experience (*now Fourteen Years*) *in Works of Intermittent Downward Filtration.* By T. Bailey-Denton. Second Edition. (London: E. and F. N. Spon, 1885.)

THE treatment of sewage by intermittent downward filtration on specially prepared areas of land is now generally recognised as the most efficient method for the purification of the sewage of towns. Mr. Bailey-Denton is one of the ablest exponents of this system, and one who has had large experience in its practical application. He is also well known as being the joint author, with Col. Jones, of a well-devised scheme for treating the sewage of the metropolis on Canvey Island at the mouth of the Thames—a scheme, however, which has not been received with any sort of approval by the Metropolitan Board of Works. The Royal Commission on Metropolitan Sewage Discharge considered very fully the merits and demerits of the system, and expressed their opinion—"(1) That the process has great scientific merit, and offers valuable practical advantages for the disposal of sewage in situations where broad irrigation is impracticable, and where land suitable for filtration can be obtained. (2) That, however, it appears desirable, when the area of land is considerably reduced, that the sewage should be previously treated by some efficient process for removing the sludge. (3) That an arrangement of this kind would be applicable to the metropolis. . . ." Broad irrigation was defined by the Royal Commission to mean "the distribution of sewage over a *large* surface of ordinary agricultural ground, having in view a maximum growth of vegetation (consistently with due purification) for the amount of sewage supplied," whereas filtration means "the concentration of sewage, at short intervals, on an area of specially chosen porous ground, as *small* as will absorb and cleanse it; not excluding vegetation, but making the produce of secondary importance." On a suitable soil—a sandy loam with a small proportion of gritty gravel to quicken percolation is the best—specially prepared by surface levelling and deep under-drainage, one acre is capable of effectually purifying the sewage—without any preliminary treatment—of 1000 people, provided that the sewage is free from any large proportion of trade or manufacturing refuse, and that storm and surface waters are kept out of the sewers. The obligation to treat storm waters, which come down in the sewers in times of heavy rain, is one of the greatest obstacles in the path of any system of sewage purification, and will continue to be until all towns are supplied with a dual system of drains and sewers. One inch of rain, thrown off 100 acres, equals 2,262,200 gallons; "and if," says Mr. Bailey-Denton, "one-tenth of this quantity suddenly reaches the outfall—say, in half an hour—no mode of treatment yet devised can deal with such a quantity without injury or defect." As a rule, at the present time, despite prospective penalties for river pollution, the mixed sewage and storm water is allowed to pass into the rivers without any sort of treatment. Mr. Bailey-Denton recommends that the storm overflow be connected with osier beds. "The beds are formed in horizontal areas which serve to *check* the rapidity of flow of suddenly discharged rainfall. This check causes the deposit of the floating solid matters in the furrows, while the flood-water rises and overflows the ridges and the osiers growing on them. These beds are not under-drained in any way; their simple purpose being to clarify those excess-waters which, without the check afforded by them, would be impetuously discharged, together with everything floating in them, into the natural streams of the watershed." Mr. Bailey-Denton does not think it necessary or even desirable, in most cases, to precipitate the sludge—the minute suspended particles, organic and inorganic, of sewage—by chemical processes or depositing tanks, before the sewage is applied to the filtration beds. He does not believe that the sludge, unless mixed with solid trade refuse, under proper treatment is capable of clogging the pores of the land or of injuring vegetation. He recommends the filtration beds to be laid out in ridges and furrows—the sewage only flowing into the latter, and not being allowed to flood the ridges on which plants and vegetables are growing. The plants cannot then be injured by the deposit of the solid ingredients of the sewage on their stalks and leaves. "As soon as the deposit of sludge on the sides of the furrows is sufficient to prevent infiltration in any great degree, the sewage is withheld from the areas so affected. The sludge is then allowed to dry (partially) in the furrows, and when in a fit condition it is lifted and dug into the ridges,—as can be seen practised at Gennevilliers (Paris). The slimy matter which had appeared so considerable, and which puddled the bottom of the furrows, when in a wet state, shrinks to a skin of very insignificant thickness when dry, and is readily broken up and mixed with the soil." Still Mr. Bailey-Denton admits that the extraction of the sludge has one great advantage, viz. that "the same land will filter double the quantity of clarified sewage liquid that it would cleanse sewage of which the finer particles have not been removed;" a very important point to towns where the area of land at disposal for sewage purposes is strictly limited.

The intermittency of the application of the sewage to the filter beds is a *sine quâ non.* Each bed should have 18 hours' rest out of the 24, to allow air to follow the sewage as it percolates through the pores of the land, thereby renewing the oxidising properties of the soil—properties largely dependent, no doubt, on the life and growth of certain Bacterial organisms resident in the superficial layers of the soil, which have been shown by Warington and other observers to be the principal agents in the nitrification and purification of the nitrogenous organic matters of sewage. The assimilative power of growing plants is doubtless also a great aid in the purification of sewage, and the plan of ridges and furrows adopted by Mr. Bailey-Denton, in enabling him to raise large crops on filtration areas, has taken away from the system the reproach that it was utterly unremunerative. There can, however, be no doubt that it is in combination with surface or broad irrigation that intermittent filtration is likely to have its most useful application. In a valuable chapter on sewage farming, Mr. Bailey-Denton points out that whatever the estimated value of sewage may be —8*s.* 4*d.* per annum per head of the inhabitants of water-closet towns, or 1⅔*d.* per ton with a dilution of 61 tons—it is actually reduced to the sewage farmer by attendant drawbacks to the present mode of application to much less than ¼*d.* per ton. The sewage must be applied to the land whether it is wanted or not, and may, under such circumstances, be the cause of mischief to crops rather than of benefit. It has been assumed that by surface irrigation one acre is capable of purifying the sewage of 100 persons; but what farmer, Mr. Bailey-Denton very pertinently remarks, would give even a farthing per ton for the obligation to apply in a year 6100 tons of liquid to an acre—equivalent to a superincumbent depth of 5 feet, or 2½ times the average rainfall—though he would gladly give a larger price per ton if he could have what he wanted, just at such times as he wanted it? "All experiences tend to prove that the obligation to 'get rid' of a large quantity of sewage under all circumstances and conditions, at night as well as day, on Sundays as well as week-days, on cropped lands as well as fallows, and at all stages of growth, from seed-time to harvest, puts it beyond the reach of man to gain any real profit from it." Many of the ordinary farm crops, as cereals, pulses, potatoes, and turnips, are injured by the application of sewage. Rye-grass, cabbages, mangolds, carrots, parsnips, and perhaps onions, are the plants that thrive best under sewage (it is said to be impossible to overdose rye-grass with sewage); but these are crops that may very readily be produced in larger quantities than there are markets for.

The great drawback, alluded to above, can be overcome by every sewage farm having specially prepared filtration areas, capable of purifying the whole sewage, when not wanted on the general surface of the farm, and leaving it within the power of the occupier to draw such quantities, at such times as he requires them, as dressings for his crops. Under such arrangements sewage farming may be expected—as it has been found by Mr. Bailey-Denton at Malvern and elsewhere—to be remunerative to the farmer and satisfactory to the town authorities. The cost of laying out the land for intermittent filtration is high—from 30*l.* to 150*l.* per acre in difficult cases, according to Mr. Bailey-Denton's estimate—but not sufficiently high in any way to counteract the immense advantages which the possession of such filtration areas confers.

A MEDICAL INDEX-CATALOGUE

Index-Catalogue of the Library of the Surgeon-General's Office, U.S. Army. Vol. VII. Insignarès-Leghorn, pp. [100] and 959. (Washington: Government Printing Offices, 1886.)

THE masterly way in which Mr. J. S. Billings is conducting this Index-Catalogue, and publishing punctually year by year these large volumes of about a thousand closely-packed pages, is a matter worth the attention not only of all interested in medicine and surgery, but also of all interested in modern libraries and modern journalistic literature. For the Library of the Office of General Robert Murray, Surgeon-General of the U.S. Army, though only founded in 1830, is now one of the largest collections of medical literature in the world, larger possibly than that of the British Museum, of the Bibliothèque Nationale of Paris, or the collections of Berlin or Vienna, and it contains some manuscripts, notably a letter of Edward Jenner's, which the English librarians would be glad to have. Its catalogue is certainly much more complete, as far as it has been published, in spite of the method of execution having been much more difficult. For these seven volumes that have been hitherto published contain more than 254,000 references to articles or essays in journals and periodicals of all kinds and in all languages, arranged under the subjects to which they refer. The French and German pleasure in framing appended bibliographies on the subjects of some monographs which they publish has never given them courage enough to face such a Herculean task. The number of periodicals which either have been or are being taken in by the Library has risen since last year, when vol. vi. was published, from 3005 to 3270, and extends through a wide geographical range, from the *Norsk Magazin* of Christiania, to the *Klin-le I-letzu* (the *Modern Medical News*) of Yedo; and in wide range of interest from the *Revue des deux Mondes*, to the *Dental Luminary* of Macon, Ga., U.S. The learned compilers of the Index-Catalogue are good enough always to translate the Japanese titles when they print them in English letters; indeed they sometimes go further, and, avoiding the difficulty of even transliterating them, give us merely the title in English, with a warning note that the original title is Japanese. Magyar is also as a rule, though not by any means always, translated; Polish sometimes, Russian only occasionally. The whole method of the book is so perfectly orderly and symmetrical, that it makes us wonder whether this want of rule in translation is one of the trifling points in which the individualism of Mr. Billings' assistants has crept in; for we cannot see that the translated titles are in any way more difficult than the untranslated.

Under the subject-headings come the great masses of quotation of the titles of articles in periodical literature which make the Catalogue so unique. If we turn to the name of an author who writes both books and papers in journals, &c., we shall not find entered under it anything but his separately published works; though, probably, all of those down to his smallest reprint from some Society's *Transactions*, and with these, in smaller type, a reference to other books to which he has contributed or which he has translated; and, if he is dead, a reference, probably, to some biography of him, and some portrait; but, beyond this, none of his contributions to journals or *Transactions*. Nevertheless, under the subject-title of any such contribution, whether it be Jaundice, Jealousy, or Jequirity, will be found a reference to his article if it was signed, and his name, in bold type, clearly standing out among the mass of contributors to that branch of knowledge. It would have been possible, of course, to print such references twice over—once under the heading of the author, and again under the subject-title; but we can hardly wonder that that has not been done, as it would have added some five or six thousand pages to a series of volumes already in danger of being overweighted, and, also, it would have supplied information which is of more importance to the biographer than to medical science.

Of the subject-titles in this volume, the largest is Labor, under which, in 150 pages, a very complete library is catalogued of some 1500 books and 10,000 articles, well arranged under many headings. Under Kidney, the student will probably be content to find references to about 400 books and 2500 articles.

It is easy to show the vast extent of the work attempted and executed; that there are absolutely no inaccuracies in the result is hardly possible, difficult as it may be to find them. The references in this volume certainly stand many tests, and most of those who have made frequent use of the previous six volumes in practical work have acquired a confidence in their accuracy which is very rare in dealing with such an immense mass of varied languages and types and abbreviations so thickly interspersed with figures.

Both the Library and the Index-Catalogue are brought fully up to date. The volume is presented to the Surgeon-General of the U.S. Army in a letter of preface dated June 1, 1886, and it contains the books and periodical references practically complete up to the end of 1885. If any comparatively modern subject is examined, *e.g.* kairine, we find nothing said or known of it before 1882, and yet 120 references, taking us over all the published literature of the subject, down to the end of 1885.

This seventh volume includes the entries from Insignarès to Leghorn. It is likely to be the middle volume of this encyclopædic work; and certainly if Mr. Billings is able to publish his last and concluding volume in 1892 no one who has any interest in the progress of knowledge will hesitate to congratulate him and his colleagues even more heartily, if it be possible, than at present.

A. T. Myers

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Sounding a Crater, Fusion-Points, Pyrometers, and Seismometers

THE account given by Prof. John Milne of his ascents and attempts at sounding the crater of Asama Yama is exceedingly interesting, and I can thoroughly sympathise with him in the difficulties he encountered, having been exposed to them on many occasions, and not always coming off so victoriously. He and Mr. Dun, however, have been forestalled by the late Robert Mallet. When I came to Naples some eight years since, I found in one of the store-rooms of the geological department of the Naples University a quantity of apparatus which I soon made out the use of.

It appears that Mallet had this apparatus made especially for the purpose of measuring the temperature and studying the gases and the lava itself within the mouth of the volcano Vesuvius. On his arrival in Naples the state of the mountain did not permit of the experiments being carried on, and the whole of the materials were left in charge of Prof. Guiscardi, who could give no other information than the above. By carefully studying the apparatus I was soon able to understand Mallet's intentions and the mechanism he intended to employ. There are two drums of small wire cable—one for traversing the crater to hold a pulley over the "*bocca*," and the other for letting down the weight and crucible. The crucible is of cast iron with a bayonet-jointed cover, and is, no doubt, intended to contain substances of different fusibilities. The apparatus is, however, an improvement on that used in Japan, in that the sounding-rope is insulated and there is an electrical bell and battery so arranged that when the crucible enters the lava it makes an earth-contact and rings the bell. There are other pieces of apparatus that I could not get at, but I think they are intended for chemical researches.

From these facts it will be seen that Prof. J. Milne has been forestalled as far as the method is concerned, but no experiments were ever carried out, either by Mallet on account of failing health, or by those in whose hands the apparatus fell. I made application to be allowed to use the apparatus, but for various reasons was unable to.

Before leaving this subject, may I appeal to your readers for a list of substances the fusion-point of which is known, and all of which would be above a dull red heat? I should like to have as complete a series as possible, so as to obtain results confined within narrow limits. Also any suggestions as to the best form of pyrometer that might be forced into and held in a stream of flowing lava, and that would not be injured by the breaking off of the rocky crust on its removal.

May I be permitted to make a few observations on the question lately raised about the authorship of certain seismographs? Not long since I described in your pages certain instruments that I considered as likely to be useful in such violent earthquakes as shake Ischia from time to time. With the exception of two, no claim was made to originality of principle, and yet I received through your pages a severe scolding from Prof. Ewing. Now if we really go into the literature of the subject we shall find the horizontal pendulum is not the invention of Prof. Ewing, but his present form of seismograph is one of the best applications of such a contrivance for measuring the horizontal component of an earth-wave, and I think he is justified in calling it *his seismograph* in so far as the present model goes. If we did not allow so much, no man using a vertical pendulum, however well contrived and modified, could call such *his*. At any rate I shall leave Prof. Milne, Prof. Ewing, and Mr. Gray to fight their own battle, but Prof. Ewing has fallen into the very same error of which he not long since accused me.

Naples, December 20 H. J. Johnston-Lavis

The Recent Earthquakes

MAY I ask to be allowed to call attention to some points in relation to the two earthquakes mentioned in your issues of December 9, p. 127 ("Volcanic Eruption in Niua-Fu, Friendly Islands"), and December 16, p. 157 ("Earthquake at Sea"). As regards the first, it is stated that "The whole island has been in a disturbed state for some three months and a half, the dates of the principal disturbances coinciding remarkably with those which are going on in other parts of the world—earthquakes on June 8 and 11, which, I think, are the dates of the first New Zealand outbreaks. . . . This is, of course, not wonderful; but the final catastrophe here took place on August 31, which, we understand, was the exact date of the recent American earthquake. It was preceded for twenty-four hours by earthquakes, and went on for ten days."

Assuming the synchronism of the Tonga eruption and earthquakes with those of the North Island of New Zealand, there is this very interesting relation between the two localities, that they both lie very near to a great circle which I may designate as the "West Coast of Africa Great Circle." This passes through, or near, the following points:—

Cape of Good Hope to St. Helena Bay; mouth of River Orange; Walfish Bay; Cape Martha; Cape Lopez Gonzalez; Bonny River; Algerian coast, near Nemours; south-east coast of Spain, near Almanzora; north coast of Spain (3° 25′ W. long.); west coast of Ireland (Loop Head); southern point of Iceland (near Westmanna Island); north-west point of Iceland; Greenland, Cape York; Melville Island (south coast and point of); Bering Island; Cape Dalhousie and coast-line of Liverpool Bay; Alaska, Montagu Island; Tonga Island (half a degree to west of Tonga Tabu); New Zealand, north-east point of North Island; passes between Adelie Land and South Victoria Land; Enderby's Land. It may be remarked that several of these localities are noted for disturbances both volcanic and seismic.

As regards the "earthquake at sea" mentioned in your number of the 16th inst., the position where it was felt is given as N. lat. 19° 21′, and W. long. 64° 22′; this gives a point about 93 miles north-east of Porto Rico. The interest in this ease lies in the fact that the antipodal point corresponding, being S. lat. 19° 21′ and E. long. 115° 38′, lies about 100 miles north by east of Barrow Island, off the west coast of Australia. It is further interesting to note that the line which joins this point with the southern point of Barrow Island fairly represents the direction of the coast-line at this point, and leads to the presumption of the existence of main lines of faulting there having that direction. To this relation of antipodean points in connection with earthquakes I have already had occasion to call attention.

J. P. O'REILLY

Royal College of Science, Dublin, December 22

Barnard's Comet

THIS comet has been observed here with the 7½-inch refractor, with power 50, than which no higher power could be used with advantage.

On December 19, at 18h., it appeared as bright as a 2nd magnitude star similarly situated; the nebulous head was about 10′ in diameter, with central condensation of perhaps half a minute. The "position" of the principal tail was estimated to be 0°: it remains of a parallel breadth throughout, and does not increase in breadth as it recedes from the head of the comet; this constant breadth is equal to the diameter of the nebulous head, that is to say, 10′. By sweeping, this tail may be traced to a distance of some 10° from the head.

The secondary tail is inclined at an angle of between 30° and 40° to the principal one, and fades away rapidly at a distance of perhaps 1° from the head; it is well defined on the preceding side, but on the following side it melts away into a nebulous mass connecting it with the principal tail for some distance from the head.

On the 27th, at 19h., the comet was decidedly less bright than on the 19th, but the same general description applies.

The "positions" of the two tails were measured, and were: the principal tail, 15°·5; the secondary, 338°; the included angle, 37°·5 ± 0°·5. The secondary tail did not appear as well defined, on the following side, as on the 19th.

Finlay's comet presents no visible feature of interest.

WENTWORTH ERCK

Shankill, co. Dublin, December 28

Electricity and Clocks

IN the absence of any details, apparently Mr. Wilson's simplest plan would be to insulate the hammer and bell of his "small striking clock," and arrange that a galvanic current should pass through both, when they come in contact by the act of striking: this current of course to be directed to a large electro-magnet, to raise the hammer for striking on his bigger bell. Should the striking of Mr. Wilson's smaller clock be on a gong with a leather-beaked hammer, a separate attachment must be made for contact.

HENRY DENT GARDNER

Lee, S.E., December 26

P.S.—If a longer contact be desired, the hammer whilst at rest should repose upon a weak spring, and be kept away from a banking; when the hammer rises, contact will ensue between the spring and banking, and last until the hammer falls again.—H. D. G.

Seismometry

THERE are one or two points in Prof. Ewing's letter on the above subject in the last number of NATURE which seem to call for a few words of reply.

(1) As to the alleged inconsistency between what I wrote in 1881 and what I wrote in my last letter. The remark quoted referred to a light pivoted frame carrying at its centre of percussion, relatively to the axis through its pivot, a pivoted mass. There was no "if need be" about this mass: it was an essential part of the system. I believe the remark I then made was perfectly correct and in no way inconsistent with my remarks in 1886.

(2) As to the vertical-motion instrument, the lever with spring joint used at Comrie in 1842 does not at all resemble the rigid lever working on knife edges and supported by springs as introduced by me and used by Prof. Ewing. On the question of compensating the lever by the addition of negative stability, I have nothing to add to what I stated in my last letter.

(3) As to the publication referred to by Prof. Ewing, the memoir printed by the Tokio University is probably, from the circumstances of its publication, hardly known to anybody. The "Encyclopædia Britannica" article is not, in my opinion, a fair account of what has been done in seismometry.

THOMAS GRAY

7, Broomhill Avenue, Partick, December 27

The Recent Weather

AT Cardross, half-way between Dumbarton and Helensburgh on the Clyde, at about 25 feet above sea-level, in an outer lobby with a temperature of say 45° to 50° F. at 9 a.m. on Wednesday, the 8th inst., the mercurial barometer stood at 27·51 inches, which, with reduction of say 0·02 added for elevation, and say 0·03 subtracted for temperature, would make it 27·50 inches. On January 26, 1884, it stood at 27·39 inches, which with like reductions would give 27·38 inches. These are nearly as low as those you refer to in your number for last week.

Cardross, Dumbarton, December 23 R. B. W.

OBSERVATIONS OF NEBULÆ AT ARCETRI[1]

M. TEMPEL observes under difficulties. The Arcetri Observatory possesses, it is true, two fine refractors by Amici, one of 11, the other of 9⅓ inches aperture; but neither is, properly speaking, available for astronomical use. The smaller is rudely set up on an open and uneven terrace, exposed to every gust of wind, and, at the most, serves to display the wonders of the heavens to curious visitors. Amici I. is duly ensconced in a revolving dome, but clockwork motion is wanting; the circles, both of declination and right ascension, are (strange as it may seem) *undivided;* and when the necessarily somewhat unwieldy instrument is, with infinite pains and without so much as the aid of a handle, pointed towards the object sought, there is actually no means of clamping it in the position so laboriously arrived at! That M. Tempel, under circumstances so discouraging to him and disgraceful to the responsible authorities, should have executed a number of valuable drawings of nebulæ, should have re-observed many such objects neglected, or even believed to have disappeared, since the elder Herschel's time, besides discovering a good proportion of new ones, gives astonishing proof of his keenness, zeal, and accuracy. All the more, nevertheless, there is reason to regret that qualities so rare should be employed at such cruel disadvantage for want of the judicious expenditure of a couple of thousand francs.

The paper before us is accompanied by reproductions of two admirable drawings by the author, one of the Orion, the other of the "Crab" nebula (Messier I.). The latter is of especial interest, as disclosing a feature unnoticed by any previous observer. This is a dark cleft right through the central condensation, dividing it along the major axis into two spindle-shaped nebulæ. Incipient fission would seem to be indicated. On the same plate with his own, M. Tempel has engraved five earlier drawings of the object, by J. Herschel, D'Arrest, Lassell, Secchi, and Lord Rosse. The comparison is instructive, if somewhat disheartening; for, assuredly, no two of the six confronted delineations could be supposed, on an unprejudiced inspection, to have been inspired by one original. Yet the nature of that original sufficiently explains the discrepancies. The apparent form of nebulæ depends upon almost evanescent gradations of diffused faint light, and differs, for each individual eye, with its sensitiveness to them. And since personal equation, as regards such gradations, is shown by many proofs to be enormously large, a vast amount of detailed variation in the representation of the objects exhibiting them becomes intelligible. It is, then, a circumstance of peculiarly good omen for progress in the study of nebulæ that a mode of record at once so fluctuating and so laborious as that of hand-drawing should be replaced (as it will no doubt soon wholly be) by automatic impressions which, with some points of inferiority, at least leave no room for "individualism."

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[1] "Ueber Nebelflecken. Nach Beobachtungen angestellt in den Jahren 1876-79 auf der Königl. Sternwarte zu Arcetri bei Florenz." Von Wilhelm Tempel. Abhandlungen der Königl. Böhm. Gesellschaft der Wissenschaften. VII. Folge, 1 Band. (Prag, 1885.)

M. Tempel's description of the Merope nebula (discovered by himself in 1859), and his remarks on the great elliptical mass in the girdle of Andromeda, derive particular interest from recent events. His observational faculty, and the high quality of his telescope, are illustrated in the disclosure to him, by Amici I., of nearly 900 stars in the Pleiades, all of them comprised within the field of view of a 4-inch Steinheil bearing a magnifying-power of 24. With the former instrument he detected independently on September 30, 1875, a few days before reading Bond's description of them, the strange obscure channels in the Andromeda nebula; and has since with some difficulty made out similar markings in some small nebulæ of the same class. They would accordingly appear to be a more or less characteristic feature of "oval" nebulæ, and might perhaps be assimilated to the symptoms of partial duplication in Messier I.

In respect to the nature of nebulæ, our author's experience leads him decidedly to adopt the view of their close connection with stars. He shows, indeed, for spectroscopic evidence a disregard that is neither philosophic nor just; yet his contention that *purely gaseous* nebulæ do not exist, is probably well founded. No aggregation of celestial mist, at any rate, has ever been observed by him in which his 11-inch failed to reveal the pricking light of minute stars, marking some knot or nucleus, and thereby evincing structural relations of a most intimate kind.

THE MATHEMATICAL TRIPOS[1]

III.

WHEN the interval between the earlier portion of the examination and Part III. had been extended to a year, it became evident that some substantial relief must be afforded to the examiners. By the existing regulations a person who accepted the office of Moderator would have to take part in the examination in three consecutive years, and in his second year of office he would have to examine the candidates of one year in Part III. simultaneously with those of the year below in Parts I. and II. This led to the consideration of the whole question of the appointment of examiners. The two Moderators in each year are nominated by two colleges, according to a prescribed cycle of fifty years. This nomination by colleges, though theoretically not very defensible, had worked very fairly so long as the examination only included subjects with which any high wrangler might be expected to be acquainted; but it was clearly unsuitable for Part III. In any case the nomination of the four examiners by four independent bodies might easily bring about the result that among the various subjects included there would be some which had not been made the object of special study by any of the examiners: indeed there was nothing to prevent the four examiners being all pure mathematicians or all physicists. Accordingly, with a unanimity almost unique in matters relating to the Tripos, the Board recommended in a Report dated June 15, 1885, that the examiners for Part III. should be quite distinct from those for Parts I. and II., and that all four should be nominated by the Board. It was also proposed that they should hold office for only one year. This Report was sanctioned by the Senate on October 29, 1886. In future, therefore, the Moderators will not take part in the highest portion of the examination. The appointment of Moderators dates from 1680. Previously the Proctors had themselves presided in the schools, but in that year the duty of conducting the disputations was transferred to the Moderators, who were specially appointed to perform this office. The Moderators have always been, and still remain, high University officers, ranking next to the Proctors.[1] Not only were they the earliest examiners in the University, but it is to them that we owe the origin of the examination system. Their severance from a portion—and that the highest portion—of the examination is therefore a notable event in the history of the Tripos. Neither the Board nor the University would have agreed lightly to such a break in the traditions of the Senate House examination, had it been possible to retain the Moderators as examiners for the final part without altering the system of nomination by colleges. The complete separation of Part III. from the earlier parts of the examination was, however, inevitable. Many members of the University who would discharge most admirably the duty of examining for Parts I. and II. would shrink from Part III.; and the professors and specialists who were best fitted to examine in Part III. would generally be reluctant to undertake the heavy burden of examining all the candidates for Parts I. and II., especially in two consecutive years.

Thus, by the irresistible pressure of events, it has come to pass, in the last few years, that not only the titles of wranglers, senior optimes, and junior optimes have lost their old significance and refer only to the earlier examination, but that even the more ancient title and office of Moderator has undergone a similar restriction. The final part of the examination has indeed made rapid progress: within three years of its first coming into existence it has emancipated itself from union with the earlier parts, and become an independent examination.

Besides these important innovations, the Senate sanctioned at the same time a slight change in the nomenclature of the Tripos, the earlier portion of the examination, previously called Parts I. and II., upon which the list in order of merit depended, being designated Part I., and Part III. being henceforth designated Part II. This change was made in order to bring the nomenclature of the Mathematical Tripos into harmony with that of the Classical and other divided Triposes.

As soon as Part II., to adopt its new name, became an independent examination, the Board directed its attention to the schedule of subjects relating to it. The existing schedule contained only those subjects which had been included in the schedule which came into operation in 1873, when the results of the whole examination were still expressed by one final list, arranged in order of merit. Now that Part II. was a separate examination, and that there was no order of merit, the reasons for the limitation of the subjects had been entirely removed. Although the theory of elliptic functions, which dates only from the publication of Jacobi's "Fundamenta Nova" in 1829, was included, the theory of numbers, which had its origin in Gauss's "Disquisitiones Arithmeticæ" of 1801, was still excluded. Abelian functions, the theory of functions of a complex variable, projective geometry, and quaternions were not formally included by name, and questions on these subjects could only be set, if at all, by straining the meaning of the title of some other subject. Besides the total exclusion of certain branches of pure mathematics, a further reason for revising the existing schedule was afforded by the fact that the four groups A, B, C, D were very unequal both in magnitude and popularity among the students. According to the existing regulations the four groups had to be equally represented by questions, and this led to a great waste of examining power, many questions having to be constructed each year upon subjects which none of the candidates had studied. The Board accordingly formed a schedule in which all the subjects of pure and applied mathematics were included—none being intentionally omitted. These were divided into eight divisions, the first four relating to pure mathematics, and the last four to applied mathematics. To avoid the waste of questions that would ensue from the examiners having to represent all the subjects in the papers in each year, they proposed that before the first day of December preceding the examination the names of the candidates and of the divisions and subjects in which they desired to be examined should be forwarded to the Registrary of the University. The examiners would thus be made acquainted with the subjects which the candidates had studied, and would be able to frame their questions accordingly. Changes were also proposed with respect to the candidates who were admissible, and to the form of the final list. By the existing regulations only wranglers were allowed to present themselves for examination in Part II., and the list was arranged in three divisions; there was no separation into *classes*, because, as only wranglers were admissible, it was considered that all the candidates were first-class men from the beginning. The new proposals were, that the restriction which admits only wranglers should be removed, and that the candidates should be divided into three classes, each class being subdivided into as many divisions as the examiners in each year thought proper. In previous schemes the endeavour of the Board had been to frame regulations that would tempt the students to specialise their reading. A few years had made so great a difference that, with a view to prevent undue specialisation, the Board now inserted a regulation to the effect that proficiency in subjects taken from more than one of the divisions should be requisite in order that a candidate might be placed in the first class.

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[1] Address delivered before the London Mathematical Society by the President, Mr. J. W. L. Glaisher, M.A., F.R.S., on vacating the chair, November 11, 1886. Continued from p. 157.

[1] It is still the custom for the list of wranglers, senior optimes, and junior optimes to be shown to the senior Proctor on the evening of the day before it is read in the Senate House. This is doubtless a relic of the fact that the Moderators were originally the substitutes of the Proctors. It has been already mentioned that the Proctors, as well as the Vice-Chancellor, used to have the right to insert a certain number of names where they pleased in the Tripos list.

The Report containing these proposals was confirmed by the Senate on May 27, and it is noteworthy that both this Report and its predecessor, in which the nomination of examiners was placed in the hands of the Board, were sanctioned without opposition of any kind. The latter of these Reports also made a few minor changes, the most important of which was the omission of the problem paper which had been still retained, from the old five days, in the scheme of 1882.[1] The examination in Part II. had assumed such a character, that the kind of questions to which one would usually apply the name of problems was no longer in keeping with the contents of the other papers.[2]

Under the new scheme, in which all the examiners were to be appointed by the Board with special reference to their collective fitness for conducting the examination in Part II., there was no further need for an Additional Examiner, and this office was, accordingly, discontinued.[3]

Thus has the Mathematical Tripos been divided into two parts; and thus has surely arisen in the University a mathematical examination of a higher type than has been known before, or could have existed under any system in which all the candidates for mathematical honours were required to be examined by the same papers throughout. For those who study mathematics for the sake of exact knowledge or mental discipline, and who propose to go forth into the world to follow professional or other careers, the first part secures all the old stimulus to industry, and gives to those who are successful the same stamp of intellectual distinction as before: such students are released at the end of their third year to enter upon the active duties of their lives, equipped with a sound understanding of the principles of the exact sciences, and with minds well trained to accurate habits in reasoning and in the acquisition of knowledge. To those whose attachment to our science lies deeper, and whose studies have carried them beyond its threshold, the second part, at the end of their fourth year, affords an opportunity of distinction of a higher kind, and one more suited to their tastes: no longer is the wise and thoughtful student hopelessly distanced in the Tripos race by his quick and ready rival.

The wants of the candidate whose mathematical career closes with the last paper in Part I., and of the candidate whose mathematical life only begins from this moment, are equally provided for by the new scheme. The order of merit relates to an examination that can bear it. All the subjects included in Part I. are such as ought to be the common property of every one who has received a sound mathematical education; and by the results of an examination in subjects which all the candidates should have read a list in order of merit can properly be formed. The specialist for the first time is set free to follow his own tastes, and give his whole heart and time to the branches of mathematics by which he is attracted. The University permits him to select any subjects he pleases from the whole range of pure and applied mathematics, and undertakes to examine him in them and award to him the credit he deserves for his attainments. A perfectly free choice is given to him, subject only to the one condition that, in order to qualify himself for admission to the first class, he must not select all his subjects from a single division.[1]

But what to us as mathematicians is more than all, as bearing on the future of our science, is that now for the first time will it be possible in Cambridge for an able and earnest worker and teacher to interest and engage his pupils in his work, and found a school such as we are so familiar with in foreign Universities, where the presence of a great professor has been almost invariably marked by a succession of illustrious pupils—pupils worthy of their master, and worthy to carry on his work. Think of the school of arithmeticians founded by Gauss at Göttingen, and how impossible such a result would have been at Cambridge, dominated as she has been by the competition for places in the Tripos! Great as has been the value to the University of the order of merit—as a stimulus to industry, an encouragement to thoroughness in mathematical study, and a paramount influence in regulating elections to Fellowships at colleges where no independent examination existed—it has yet been in recent years a deadly enemy to the spread of research and the advance of our science. Throughout his whole career the student has had to devote himself unremittingly to the work for his Tripos, taking up a fresh subject each term, and often having to read two in one term. He could never pursue any subject far enough to reach the really interesting portions of it, or obtain complete command over its methods: he was always occupied with something new, starting afresh and gaining familiarity with new principles, new processes, new modes of thought. Many of the higher lecturers in the University were necessarily neglected by the students: they could pay but scant attention to any subject which was not adequately represented in the Tripos, and even in the case of the subjects which were so represented they were tempted to pass lightly over those investigations, however important, which from their length and character were unsuitable for reproduction in an examination. Now, however, all this is history. When a good course of lectures upon any high subject is given in the University, those students who have attended the course will send in that subject as one on which they desire to be examined: it will, therefore, be properly represented by questions; and the subject will become one that will be increasingly studied year by year. It will now be possible for any capable mathematician, by means of his lectures, to gather pupils round him who will bring his subject into prominence, and make it one of special study in the University.[1] It has been said that in mathematics we have in England generals without armies: the great men who are independent of circumstances have arisen among us, but where are the rank and file? It is my belief that the great obstacle to the existence of the rank and file has now been removed.

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[1] Although the Board were unable to make any recommendation upon the subject, I may mention that the principle of prefixing to the final three days a preliminary day, in which the subjects of examination should be those parts of higher pure mathematics which are needed in mathematical physics, found a considerable amount of favour on the Board. The proposal, however, was found to be more difficult in execution than was anticipated (partly on account of the impossibility of forming a perfectly satisfactory schedule of subjects for this day), and was ultimately abandoned by most of its original supporters.

[2] In the first examination in Part III., in 1883, the examiners set, as one of the question papers, a paper of essays; and their example was followed by the examiners in 1884, 1885, and 1886. These essay papers were introduced merely on the authority of the examiners, and not in consequence of any new regulation. Experience seems to show that the essay paper affords very little additional assistance in ascertaining the relative merits of the candidates. The essays were, perhaps, more useful at first, when they were a novelty.

[3] Unless the office should be revived at some future time, there will therefore have been only one Additional Examiner for the final part of the examination, viz. in 1886, the last occasion of the examination taking place in January.

[1] This condition would be complied with by the candidate's showing proficiency in one subject taken from one division and in one other subject taken from one other division. The intention of the Board was to discourage students from specialising too narrowly at too early a period. Some of the divisions (as, for example, the fourth, which contains only projective geometry and analytical geometry of curves and surfaces) are so restricted that it was considered undesirable that students should be allowed to confine themselves entirely to subjects chosen from a single division.

Whatever else it may be, Part II. is at all events a "limiting form." No wider choice of subjects could be given to the candidates; no greater freedom to the examiners. The schedule of subjects includes all mathematics: the examiners may issue any kind of list. By introducing numerous divisions into the classes they may make it approximate as closely as they please to an order of merit; or, on the other hand, they may make it merely a class list. They are empowered to give to their list just such a form as they feel justified in doing by the results of the examination. In the appointment of examiners, also, the limiting form has been reached, all four being nominated by the same University authority, and holding office for one year only.

With respect to Part I., it may be that the ultimate form has not yet been reached. There are some who think that, as in some other Triposes, the students should have the option of becoming candidates at the end of their second year. It would seem, also, that the range of subjects is rather too restricted; and, as may be inferred from what I have said near the beginning of my address, I should myself like to see the elementary portions of elliptic functions included in the schedule of Part I. Still, these are but minor points; and I think that the principle of subjecting all the candidates for mathematical honours to one and the same examination in comparatively elementary subjects, and arranging the list in order of merit, meets with general approval.

A few years ago, when the old Tripos was exerting its stifling influence upon the higher mathematical studies of the Universities, I felt disposed to welcome the abolition of the order of merit as the lesser of two evils; but now that the Tripos is divided, and that the mathematician has his own examination especially framed for him, I should be sorry to see a modified class list substituted for the order of merit in Part I. A severe competition for places has the great advantages of keeping the candidates closely employed, and extracting from them their best work. At present an immense amount of thoroughly good mathematical work is done in the University. We have received from our predecessors a system under which the principles of mathematics are efficiently taught, the powers of the students are exerted to the utmost, and upwards of a hundred persons each year receive a mathematical education which is in some respects unique. These are substantial advantages which should not lightly be jeopardised or exchanged for others that are problematical. Under any other system I think the quantity and the quality of the mathematical work done in the University would suffer. It should be remembered also that there is no subject in which the knowledge of a candidate can be so readily tested by examination as in mathematics, and that in no other subject can the results of an examination be expressed with such certainty and accuracy by an order of merit.

I believe there are indeed but very few who have graduated in the Tripos who would set a slight value upon the advantage which their mathematical training has been to them throughout life; and on the other hand I think that it has been an indirect benefit to our science that among those who have won distinction in public and professional life there have always been some—and those not the least influential or eminent—who have passed through an extensive and thorough course of mathematical study, and to whom our world of symbols is no *terra incognita*. The fact that our results, unlike the conquests of astronomy and other branches of applied mathematics, can only be expressed by means of a language of their own, requiring years of study, imposes of necessity such narrow limitations upon the numbers of our audience that we cannot be insensible to the advantages of any system by which the power of understanding and appreciating the beauties of our science is extended to others external to our own ranks. Under the new scheme these advantages are still retained; and, difficult as is the problem of combining a mathematical course for the many with the technical requirements of the few, I believe that a satisfactory solution has rewarded the efforts of the last twenty years. I believe that the University of Cambridge will become a great centre of mathematical research and a home of the exact sciences, and that it will be found that these objects have been attained without any sacrifice of the general efficiency of the training received by the bulk of the candidates for mathematical honours.

On taking a survey of the history of the Tripos during the last half century, perhaps the feature that stands out most strongly is the part played by the subjects of electricity and magnetism—their half-recognised existence before 1848, their exclusion until 1873, and the effects which followed their restoration in that year. It was the extension of the dominion of mathematics over these great and growing branches of physical science that broke down the old system. Electricity and magnetism became too important to be excluded; but when included the examination in its old form was too heavily weighted to exist.

The year 1877-78, in which the syndicate of 1877 was endeavouring to frame a scheme that should relieve the strain of the excessive competition without sacrificing the order of merit, was perhaps the most eventful period in the whole history of the examination: it then became evident that it was impossible to retain the existing system even in a modified form, and that a complete reorganisation of some kind was inevitable. Although the frequent changes in the last few years have been productive of some inconvenience, I think it is fortunate that the syndicate was so reluctant to propose any sweeping changes, and that the present scheme has come into existence as it has done—not as the work of any influential legislator, but as the form which the examination has of itself assumed under the pressure of the actual forces at work in the University. The order of merit for the whole examination was not given up till it was clearly shown that its retention was an impossibility; and, on the other hand, Part II. has grown up by a process of regular development, and been moulded into its present form by those most interested in promoting the higher mathematical studies of the University.

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[1] In the schedule for Part II. no subjects are ignored or favoured less than others, so that by the new scheme provision is made for the growth of any subject which may happen to take root.

Special reference also should be made to the "three days." It will be remembered that this preliminary portion of the examination was the principal feature of the scheme which came into operation in 1848. Both the subjects and the methods of solution that may be employed are defined by a schedule, and only those who satisfy the examiners by their performances in these three days are admitted to the subsequent parts of the examination. It is very singular that an arrangement devised so long ago should not only still continue in force, but even be regarded by some as the most thoroughly satisfactory portion of the whole system. The framers of this scheme and schedule might well have been proud of the lasting character of their work if they could have known that it would outlive two sets of University statutes, and, amidst changes on every side, remain unchanged for forty years.

The early history of the Tripos and its gradual development into an examination by written answers, and finally by printed papers also, are especially interesting in these days, when the merits of the examination system are so highly appreciated, and its adoption is so universal. The Senate House at Cambridge is the cradle of the modern form of examination in England.

In connection with the Tripos there is one matter of so much importance that I cannot pass over it entirely without mention. I mean the influence of the system of private tuition. I believe that while there is an order of merit it will always be a great assistance to the majority of the candidates to read with a private tutor. Mathematics is a difficult science; and when a considerable range of subjects has to be traversed in a comparatively short time, and the knowledge of the candidate has to be finally tested by an examination such as Part I. of the Tripos, it cannot fail to be a great advantage to him to have his difficulties explained, his path smoothed, and his skill in working out problems developed, by an experienced private tutor specially interested in his individual welfare. The system of private tuition has been objected to from two points of view: (1) because it is unsatisfactory that the instruction which is valued most highly by the student should be received from his private tutor instead of from college lecturers or University professors; and (2) because the student who has followed implicitly, during his whole undergraduate career, the minute directions of one man with regard to his reading, is placed after his degree, when he is deprived of his guide, in a very unfavourable position for pursuing further his mathematical studies. The first objection does not concern us here: my own feeling is, as I have just said, that, whenever an order of merit exists and the competition for places is keen, the services of private tutors will necessarily be called into requisition. The second objection is one which is of far more importance to our science. There can be no question that, brilliant and eminent as have been the greatest of the private tutors at Cambridge, one result of the system has been that many of the ablest students have been left after graduation not only without any knowledge of the way to follow up the study of the subjects of which they had learned the elements, but even without any taste or inclination to do so. The private tutor's manuscript and verbal instruction had superseded all need of referring to the original memoirs, and the nascent wrangler knew nothing of the great world of mathematical literature or of the modes of reaching it. On the other hand, it is only fair to say that the amount of mathematical knowledge acquired by the best pupils from their private tutors in the course of their undergraduate career was really wonderful; and that till quite recently neither the University nor the colleges offered any inducement to the mathematical student to continue his reading after the Tripos. The fact that the student's horizon should have been bounded by the Tripos, and that his training should have been directed with the view to giving him skill in working out questions rather than to developing his taste for the science he was studying, was principally the fault of the system as a whole; but it was certainly intensified by the complete subjection of the pupil to the course of reading placed before him by the private tutor. A student whose interests and aspirations had been at least held in check, and perhaps entirely stifled, throughout his whole undergraduate career, was generally too subdued or helpless to be able to make use of his freedom when the examination was over.

Under the new scheme the private tutor still occupies in the main his old position with respect to Part I., although, of course, the higher places in this examination have much less significance than before. With respect to Part II. it is quite different. No attempt is made by private tutors to teach these higher subjects, which, both from their character and extent, are clearly unsuited for private tuition; and the students are compelled to rely upon the lectures delivered in the colleges and University in their preparation for this final examination. Thus, in their fourth year they are brought into contact with the leading mathematicians in Cambridge; and when the examination sets them free to pursue their own studies or researches, they start on their new career fresh from the best teaching which the University affords.

Although the subject of my address is the Mathematical Tripos, it may be regarded as still falling within my province to refer to other changes that have taken place in the University for the purpose of encouraging original mathematical work. Fourteen years ago Trinity College invited mathematical candidates for Fellowships to send in, before the examination, dissertations upon any subjects of their own selection. It was announced that these dissertations, if possessed of decided merit, would be taken into account in the Fellowship election, together with the results of the Fellowship examination. Not only have these dissertations been of the greatest value in guiding the choice of the electors, but many of them have been important contributions to mathematical literature.[1] The example of Trinity College has been recently followed by St. John's College and King's College. The Smith's Prizes, which for a great number of years had been awarded by a special examination, are now awarded annually for mathematical dissertations, the candidates being free to select their own subjects. This new scheme passed the Senate on October 25, 1883, and the first award of the prizes under it was made in 1885. Powerful inducements are, therefore, now held out by the University and some of the colleges for the best students to devote themselves to original work. The importance to our science of these direct incentives to research cannot be overestimated. They come into operation as soon as the stimulus of the examination is removed, and, instead of resting upon their laurels, the ablest mathematicians of the year are induced to concentrate their powers upon a single subject, just at the time when they are undaunted by any amount of hard work, when their stock of general mathematical knowledge is freshly acquired, and when their minds are flexible, vigorous, and free from care. It is indeed strange to look back upon the changes of the last few years, and to contrast the encouragement now given to mathematical research with the indifference, or even worse, of twenty years ago.[1]

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[1] Among the Trinity dissertations which have subsequently been printed, I may mention the late Mr. R. C. Rowe's "Memoir on Abel's Theorem" (*Phil. Trans.*, 1881), Mr. Forsyth's "Memoir on the Theta Functions" (*Phil. Trans.*, 1882), Mr. Homersham Cox's "Application of Quaternions and Grassmann's Ausdehnungslehre to different kinds of Uniform Space" (*Camb. Trans.*, 1882), Mr. Gallop's "Distribution of Electricity on the Circular Disc and Spherical Bowl" (*Quart. Math Journ.*, 1886), and Mr. R. Lachlan's "Systems of Circles and Spheres" (*Phil. Trans.*, 1886).

I cannot close my address without saying a few words upon our Society. We were founded in 1865, and so to-day we attain our majority. I think we can safely say that we have steadily and uniformly kept to our single purpose of promoting the advance of mathematics. We have published seventeen volumes of *Proceedings*, and every paper we have printed has been subjected to a rigorous examination by two referees. We already have a history to look back upon: familiar presences among us—De Morgan, Clerk Maxwell, Clifford, Henry J. S. Smith, Spottiswoode—have passed away; and for most of us this very room is full of associations with those whom we shall see no more. I should like before concluding to formally express our gratitude to our two Secretaries, Mr. Morgan Jenkins and Mr. Robert Tucker, who have served us so faithfully almost from our foundation, and to whom the successful development of our Society has been largely due. I will not utter any aspirations with regard to our future: we shall never be a great Society in numbers, but we can continue to do what we have done, and to spare no effort to encourage the advance of mathematical science.

THE INTERNATIONAL COMMITTEE OF WEIGHTS AND MEASURES

THE results of the scientific investigations made under the directions of the Comité International des Poids et Mesures at their Bureau at Sèvres during the past year are stated in vol. v. of their "Travaux et Mémoires," recently published (Gauthier-Villars, Paris, 1886) under the authority of the Director of the Bureau. This volume contains the following papers:—"Note sur l'étallonage des sous-divisions d'une règle, sur l'étude des erreurs progressives d'une vis micrométrique, et sur le calibrage des thermomètres," by Dr. O. J. Broch; "Études thermométriques," by M. Ch. Ed. Guillaume; "Études sur la balance," by Dr. M. Thiesen; "Sur quelques analyses chimiques faites pour le Bureau International," by M. Tornöe. The two latter memoirs, however, are published under the responsibility of their authors.

In the method of calculating the errors of the subdivisions of a standard measure of length, or of calibrating a thermometer, Dr. Broch has followed the celebrated astronomer P. A. Hansen; but he has endeavoured to render Hansen's method more simple; and he has abbreviated it and reduced the number of observations, without increasing the probable error of the results obtained. Convenient tables of equations are given for the more ready application of Hansen's formula, and also examples of an abbreviated method for calculating the several lengths of the decimetres, centimetres, and millimetres on a subdivided standard metre.

The second part of this Note deals with the progressive errors of the micrometer-screw. As each interval to be measured on a linear standard is contained within two lines, we have to pass by successive turns of the micrometer-screw from one line to the other. Each line in turn is bisected by means of cross or of parallel webs; and not only do the personal errors of bisection have, of course, to be allowed for, but even small errors in the micrometer-screw itself have to be corrected. Examples of such corrections are given in this note. In considering the progressive errors of micrometer-screws, we are not sure that Dr. Broch has sufficiently, however, investigated the variation in the amounts of such errors owing to the wear of the screw.

In the third part of the note is given an explanation of an abbreviated method of calibrating graduated glass tubes or thermometer-stems, and of applying corrections to the calibrated lines. The method of interpolation by differences is also discussed and simplified.

Dr. Guillaume, in his "Études thermométriques," continues the thermometric work which was begun by Dr. Benoit, and by Pernet at this Bureau. It is required of all standard thermometers verified at the Bureau, that they should carry the fundamental points 0° and 100° C.; that they should have a total length of as much as 70 centimetres, the diameter of the stem varying from 3·5 to 5·5 millimetres; and that each division should be nearly 5 millimetres in length. The testing of the thermometers includes the three distinct operations:—

(1) Division and calibration.

(2) Determination of the coefficient of pressure (when the thermometer is placed alternately in a vertical and in a horizontal position).

(3) Determination of the fundamental points and of the mean value of each degree.

It is with these operations, as well as with the actual verification of certain standard thermometers at the Bureau since the year 1883, that Dr. Guillaume now deals. The paper is an interesting one, and all the observations are printed in the fullest detail. Particularly in that part of this paper which discusses the variations in the readings of thermometers by time and circumstance, there is much to be learnt.

Of late, attention has been given, especially in Germany, to the kind of glass best adapted for thermometers, the zero-points of thermometers made of some kinds of glass, being found less likely to alter by age like ordinary thermometers. A careful analysis made by M. Tornöe of the glass used for the bulbs of two of Tonnelot's thermometers used at the Bureau, gave the following results:—

	Hard glass	Plate glass
Silica	71·52	60·68
Sulphuric acid	0 72	0·37
Chlorine	traces	—
Peroxide of iron	0·22	—
Lime	14·55	5·44
Soda	10·81	10·50
Potash	0·37	6·55
Magnesia	traces	traces
Protoxide of manganese	traces	—
Oxide of lead	—	15·12
Alumina	—	—
Alumina, with traces of iron and manganese	—	0·87

The analyses of the stems of the thermometers showed somewhat different results.

The memoir by Dr. Thiesen, "Études sur la balance," continues the excellent work on the construction and use of the balance which was originally begun at the Bureau by M. Marek. In the "Théorie générale de l'équilibre statique de la balance," and in the "Calcul de l'équilibre de la balance," Dr. Thiesen has discussed the conditions which affect the equilibrity of a balance, and also has investigated the effects of outside influences during weighings, as those arising from currents of air and from changes in the condition of the air; and from electrical disturbances, magnetic and radiometric. A good balance may be relied on to 0·001 mgr. in the comparison of two standard kilogramme weights, but outside influences increase the probable error to ± 0·004 mgr.

The labours of the Bureau have been particularly devoted to the perfecting of existing methods, and they have resulted in the attainment of far higher accuracy in weighing and measuring than was thought to be possible, or necessary, even ten years ago. We trust that the labours of the Comité may soon be crowned by the completion of the international metric standards of length and weight, for which all their present investigations are preparatory.

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[1] Under the old system, the Cambridge graduate who devoted himself to mathematical research possessed one advantage over his Continental colleagues in the wider range of his general mathematical knowledge. Although Part I. is considerably more restricted than the Tripos of 1848-72, this advantage is still retained to a substantial extent.

NOTES

Mr. C. L. Griesbach, lately geologist to the Afghan Boundary Commission, and deputy superintendent of the Geological Survey of India, has been appointed by the Viceroy to officiate as superintendent.

The annual meeting of the Association for the Improvement of Geometrical Teaching will be held on Friday, January 14, 1887 (11.30 a.m.) at University College, Gower Street. At the afternoon meeting (2 p.m.) the following papers will be read:—"On the Teaching of Modern Geometry," Rev. G. Richardson; "The Modern Treatment of Maxima and Minima," Rev. J. J. Milne; and on "Geometry from an Artist's Point of View," G. A. Storey, A.R.A. The meetings are open to all who are interested in the objects of the Association.

Our readers will be interested to hear that at the meeting of the British Association recently held in Birmingham, a movement was originated in the Committee of Section D (Biology) having for its object an application to Government for a small grant out of the Civil List to Mr. Thomas Bolton of Birmingham, whose important services to science as a naturalist and microscopist have long been well known and appreciated. A memorial setting forth Mr. Bolton's claims was prepared by Mr. W. R. Hughes, late President of the Birmingham Natural History and Microscopical Society, and was signed by Sir J. W. Dawson, the President of the British Association, and by a large number of eminent men of science. It also received the signature of the Mayor of Birmingham. The memorial was recently presented to Lord Salisbury as First Lord of the Treasury, who has recommended that Her Majesty grant Mr. Bolton a Civil List pension of 50*l.* per annum.

The finest of all Japanese botanical books is the Honzo Dsufu. It is also from a scientific point of view the most valuable, inasmuch as it contains excellent coloured figures of no less than 1500 species of Japanese plants, of many of which there are no other published representations. Franchet and Savatier, in their "Enumeratio plantarum in Japonia sponte nascentium," quote throughout the copy in their possession, which was not, however, quite complete. It is in ninety-six volumes, or rather *livraisons*, and is rare even in Japan. It was prefaced in 1828, but only the first six *livraisons* have ever been printed, and the rest only exists in hand-made copies. It has long been desired to obtain a copy for the library of the Royal Gardens, Kew, and this wish has at length been gratified by the kind liberality of Mr. Tokutaro Ito, grandson of the well-known Japanese botanist, Keisuke Ito. Mr. Ito is now studying botany at the University of Cambridge, and lately communicated a revision of Japanese *Berberidaceæ* to the Linnean Society, of which he has recently been elected a Fellow. The Kew copy of the Honzo Dsufu is probably the finest to be obtained in Japan. It came from the library of Senator Tanaka (himself a distinguished botanist), who, with extraordinary generosity, placed it at the disposal of Mr. Ito for presentation to Kew.

In the *Annalen* of the Vienna Natural History Museum, Herr von Pelzeln and Dr. von Lorenz have just published the first of a series of articles on the types of birds contained in that Museum. This cannot fail to be of the greatest use to students, who often require to know the present resting-place of typical specimens. Following the Cuvierian arrangement as adapted by Gray in his "Hand-list of Birds," the authors present, as a first instalment, a list of the types of the *Accipitres* and *Tenuirostres*. The chief interest naturally centres round the species procured by Johann Natterer in Brazil, for nothing more wonderful is known in the history of ornithology than the way in which Natterer's collections, made in the early part of the present century, still remain the basis of our knowledge of the ornithology of that country, and, notwithstanding the subsequent efforts of travellers, there are numbers of Brazilian species obtained by Natterer alone, and unrepresented in any Museum except that of Vienna. Curiously enough, too, the Vienna Museum also possesses several of Latham's and Shaw's types, founded on the specimens in the Leverian collection, and purchased in 1806. The value of a type was not understood in England so long ago as 1806, and the specimens were allowed to leave the country, to find a home in Austria. Such would scarcely be permitted now, under the enlightened management of our authorities at the Natural History Museum at South Kensington, who are doubtless mindful of the disgrace attaching to the British Museum in former years, when that institution allowed the whole of the Gould collection of Australian birds, with its 300 types, to go for 1000*l.* to America, where it now lies, scarcely heeded, in the Museum of the Academy of Natural Sciences of Philadelphia. Let us hope that the Gouldian types are better looked after in the Philadelphia Museum than some of the types of Du Chaillu's Gaboon species, which are no longer forthcoming, to the no small embarrassment of ornithological students.

Prof. Menzbier has recently published, in the *Bulletin* of the Society of Naturalists of Moscow, an account of the birds collected by Mr. Zaroudnoï, a Russian naturalist, who has been exploring the oasis of Akhal-Tekké, the Kara-Kum desert, and the adjacent mountains, in Central Asia. The want of funds appears to have crippled the efforts of the traveller to a great extent, but he managed to procure 184 different species of birds, though his observations were confined to the summer months and early autumn. Mr. Zaroudnoï found several rare species nesting, and besides his own observations there are some interesting scientific notes from Prof. Menzbier's pen. We are informed that the traveller has recently prosecuted a further expedition into Khorasan and Northern Afghanistan, the results of which may be expected to be of considerable importance to zoologists.

The *Auk*, which is the journal of the American Ornithologists' Union, and answers to our English *Ibis*, has just completed its third volume, under the able editorship of Mr. J. A. Allen, who is the President of the American Ornithologists' Union. The present volume abounds in interesting memoirs, and fully maintains the high standard of the journal. The Union now numbers 46 active members, 112 associate members, 26 foreign members, and 59 corresponding members. The Committees on the Migration and Geographical Distribution of North American Birds and for the Protection of North American Birds have both done excellent service during the past twelvemonth.

Mr. G. H. Hinsby, of Hobart Town, has forwarded us a useful list of the birds of Tasmania: 178 species are found in the island, but the author is apparently unaware that several Tasmanian birds to which he gives the same scientific name as the Australian species are considered by recent writers to be peculiar to Tasmania itself.

A recent issue of the *Japan Weekly Mail* contains a report of the Japan Educational Society, an association founded to bring together persons interested in education, to assist in its diffusion, and to improve and advance education in the country. Besides general and ordinary meetings in furtherance of the objects of the Society, members are frequently sent to various localities at the request of local educational institutes for the purpose of delivering addresses or lectures. Thirty-three numbers of the memoirs have been published, the total number printed being 100,000. In addition, books under the title "Hints to Educators" were published, and 7000 copies printed. The number of members is 3000, and a prince of the Imperial House is President.

UNDER the title of a "Descriptive List of Native Plants of South Australia recommended for Cultivation," Mr. J. G. Otto Tepper, F.L.S., has reprinted in pamphlet form some notes that apparently appeared periodically in Adelaide. As a reason for publishing the list Mr. Tepper says:—"At the rate South Australia and its sister colonies are progressing in civilisation, the time can easily be foreseen when for long distances from any centre of population not a mark would be discoverable where any one could view the native vegetation in its natural state. Owing to the very local distribution of many Australian herbs, shrubs, and trees, there is even the possibility that they may be entirely extirpated, caused by ruthless and ill-judged clearing, depasturing of domestic animals, choking by introduced weeds, and the diminution of the moisture in the soil by the first two causes. Few attempts are made to cultivate any, so far as we know, though a few (for example *Kennedya monophylla*) have already found their way to the favour of gardeners, who, perhaps, do not even know that these plants are indigenous." The list consists of a number of plants belonging to very different natural orders and of very different characters, such as herbs, trees, shrubs, &c., as may be instanced by species of *Mesembryanthemum*, *Viola*, *Acacia melanoxylon*, &c., &c. To each plant its habit and size are given, a short description of the leaves and flowers, time of flowering, and nature of locality where found. The descriptions, however, are by no means equal in point of detail, some being considerably longer than others. To some of the plants the natural orders are stated, while to others no mention whatever is made. No references are made to the uses of the plants, and no kind of arrangement of genera has been adopted, either scientifically, alphabetically, or in any other way. The list may be of use to those for whom it has been written, but it would have been more valuable if some arrangement had been adopted by which any given plant could have been found without wading through the whole nineteen pages.

MR. STEVENS, the Queensland naturalist whose visit to the Veddas of Ceylon we have already mentioned, addressed a recent meeting of the Asiatic Society of Ceylon on this little-known people. He found the time at his disposal on his first visit too short to investigate satisfactorily the problem of their origin, but he intends going amongst them for another six months on his approaching return from India. He has offered to live with them for a year or two if such a long absence from his other duties can be arranged. He regards the popular notion in Ceylon of the Veddas as a cruel, vindictive, suspicious people as wholly erroneous. He found them truthful, hospitable, and honest, but they exhibit a marked aversion to Singhalese and Tamils. They are very peaceful, and hence a European can travel amongst them in perfect safety and freedom. They have a language of their own which the Singhalese do not understand, and of which he collected a considerable number of words for examination by Oriental scholars. They are expert archers, and can send an arrow completely through a wild animal. Mr. Stevens would prefer to face a rifle in the hands of an experienced person at fifty yards' distance rather than a Vedda armed with his bow. They have no idea of boiling anything; they use the fire-drill, and they appear to have had sufficient knowledge of working in metals to supply themselves with weapons. He questions whether there are 500 Veddas in all Ceylon, so that soon it will be difficult to find a real one. Hence he urges the great importance to science of a thorough study now of their language and habits. Demonology is, he thinks, an incorrect term to apply to their religion; it is, rather, "Kapuism." They do not believe in the existence of any injurious or malevolent spirits. Once a year the whole of a Vedda encampment make a propitiation; it is not worship, but simply a propitiation to the eight or nine gods of their pantheon. They divide themselves into eight clans, which rank in a kind of social gradation, depending, in some instances, apparently on their traditional origin. He obtained skulls of representatives of seven of these clans. Throughout the address, Mr. Stevens constantly insisted on the tentative nature of his investigations so far. His facts, "or, rather, supposed facts," are entirely unverified. They require assortment and further examination, and he urges societies and students in Ceylon to undertake the work. It is greatly to be hoped that Mr. Stevens himself may be able to carry out his project of residing amongst the Veddas for a prolonged period, and studying them from the inside, and, in a certain degree, as one of themselves. One of his facts requires no verification, viz. that he can live and travel amongst them with safety, and that he has the capacity for making friends of them.

M. DE QUATREFAGES, at a recent meeting of the Geographical Society of Paris, advanced the theory with regard to the migration of peoples at a remote period of antiquity, which, at a subsequent meeting, was discussed and approved by M. H. Chevalier. The theory is that these migrations were due essentially to the gradual increase of cold in the northern regions, which forced the inhabitants to wander to the south in search of a more temperate climate. M. Chevalier quoted certain passages from the Zend Avesta, which, he argued, corroborated this theory.

A PROSPECTUS has been issued by the Council of the "Lochbuie Marine Institute" on the Isle of Mull, recently established under the auspices of the National Fish Culture Association, setting forth their objects. One of these is to incubate herring-ova to re-stock such locations in Scotland as have been depleted of that fish through the action of fishermen in exhausting the supply under the belief that they were general instead of local. Meteorological and other observations are also to be carried out under the direction of Mr. Anderson Smith.

DR. FOREL sends us the following list of recent earthquakes in Switzerland:—December 16, 16h. 0m., at Sarnen (Unterwalden); 22, 0h. 3m., 4h. 20m., and 5h. 30m., at Pontresina (Grisons), all Greenwich time.

WE have referred on several occasions to the extraordinary number of rats which emerge from various parts of the building when the late Exhibitions at South Kensington have closed and the supply of food is cut off. This year their number has been larger than ever, and shortly after the termination of the late Colonial and Indian Exhibition the rats, desperate with hunger, invaded every part. During the summer nothing would induce them to enter traps, whereas now they rush in as fast as they are set, and not until they have devoured the bait do they seem to realise the fact that they are prisoners, when they seek deliverance in their usual wild fashion. During last week their cravings for food culminated in a fierce onslaught upon one another, which was evidenced by the piteous cries of those being devoured. Their method of seizing their victim is to suddenly make a raid upon one weaker or smaller than themselves, and after overpowering it by numbers, they tear it in pieces. At the present time there cannot be found a single young rat in the building. So far this is satisfactory, as the large numbers bred during the summer will thus become exterminated.

AT present the city of Worcester possesses a public free library and natural history museum in one building, and a Government school of art in another. It is proposed to celebrate the Queen's Jubilee by establishing an institution to be called the Victoria Institute in a central position in the city, in which the existing library, museum, and school of art will be placed, and to unite with them in the same building a school of science and an art gallery. The cost is estimated at 18,000*l.*, of which the Corporation have voted 7000*l.*, and the old site and other sources of income will leave only about 5000*l.* to be raised by public subscription. A considerable portion of this amount has already been raised.

A LATE issue of the Batavia *Dagblad* contains a report of a paper read by Dr. Cornelissen, of the Java Medical Service, before the Society for the Advancement of Medical Science of Java, on his researches in Acheen, in Sumatra, into the causes of the dreaded disease *beri-beri*, known as *kakke* in Japan—a species of elephantiasis. Dr. Cornelissen comes to the unexpected conclusion that it is infectious, and is propagated by bacilli. He accordingly recommends a thorough system of disinfection in hospitals and troop-ships where patients suffering from this malady have been kept. The theory has caused much excitement in Java and the neighbouring regions where the disease prevails, for it has not hitherto been suspected that it was infectious. Dr. Cornelissen's theory, however, does not appear to be generally accepted in Java and the Straits Settlements.

IN the current number (27) of *Excursions et Reconnaissances* of Saigon, M. Aymonier brings to a conclusion his notes on Annam, the particular province dealt with being Khanh-Hoa. The most interesting part of the paper is the sketch of the so-called savages, or Mois, inhabiting the mountains of the province. These papers have now been running through many numbers of the periodical, and are encyclopædic in their nature. M. Aymonier is, beyond question, the greatest living authority on Cochin China generally, and he undertook prolonged journeys into various parts of the country with a view to perfecting his information for this series. His original intention was to explore the whole coast of Annam up to Tonquin, but the rebellion of 1885, which resulted in frightful massacres of missionaries and native Christians, prevented him from carrying out this project. Accordingly in his "Notes" he has been compelled to omit all reference to the ancient kingdom of Ciampa, as well as to a great part of Annam, and to confine himself to the two great southern provinces Binh Thuan and Khanh-Hoa, which stretch from Ciampa on the coast across to Cambodia. Capt. Réveillère, who has already twice navigated the Meikong rapids in a gun-boat, describes a voyage on that river in a steam-launch. The Meikong can scarcely be said to be a new river to geography, inasmuch as the greater part of its course was described with great minuteness in the work recounting the details of La Grée and Garnier's expedition from Saigon along the Meikong to the Yangtsze, published ten or twelve years ago. Father Azemar describes the Stiengs, amongst whom he lived between 1861 and 1866, and gives a vocabulary of their language. The Stiengs form one of those wild tribes which inhabit the mountains between Cochin China and Tonquin on the east and Laos and Siam on the west. The writer thinks they have no ethnic affinity with the Mongol family, mainly basing his opinion on differences in language and manners.

PERHAPS the most important point to be noticed about the Perthshire Society of Natural Science, the *Proceedings* of which for the past year we have received, is that the present method of publication has been abandoned. For six years past the *Proceedings* have, for the most part, been reprinted from the reports of the meetings which have appeared in a local newspaper. But the selection and arrangement of matter most suited to a newspaper were not always the best adapted for the *Proceedings* of a scientific Society. The Council have, therefore decided to commence a new series of *Transactions* and *Proceedings*, which will be specially printed for the Society, under the supervision of a publishing Committee. An examination of the *Proceedings* now before us certainly reveals so much activity in many departments of research that the Council appear justified in this resolution. It is especially noticeable that the papers read refer, almost without exception, to local investigation —in our judgment the most valuable and instructive work in which the members of such a Society could systematically engage. Thus, we have some notes on a collection of nests and eggs presented by a local landowner; a thorough description, by several hands, of the natural history of Kinnoull Hill, under the heads of Introductory, Geology, Flowering Plants, Ferns, Mosses and Fungi, Insects, Mollusca, and Vertebrates, and many others of the same kind. Dr. Buchanan White's address this year, as last, urges the improvement of the museum, with a view to securing more space for the exhibition of the collections. He dwells on the value of a properly selected and arranged museum as an educational medium for the members of the Society, and, quoting the words we used last year in regard to this subject, that a local museum, to be of the fullest value, should be made as complete as possible, he explains what degree of completion he expects such a museum to attain.

WE have received the *Proceedings* of the Holmesdale Natural History Club, with its home at Reigate, for 1884 and 1885. The papers are of a very general kind, ranging from the continuity of protoplasm to the wild animals of South Africa, and from mahogany to the Yellowstone National Park. Students will probably turn with most interest to two papers by Mr. W. H. Beeby on recent additions to the flora of Surrey, Mr. Tyndall's meteorological notes for the two years, and Mr. Crosfield's paper on the geographical distribution of wild plants in the British Isles.

THE Town Council of Bombay has unanimously resolved that the municipality must bear its share, with the Government and other public bodies, in the expenses of the establishment of a technical school, and a sum of 5000 rupees was voted for the purpose at a late meeting. The scheme is one drawn up by Dr. Cooke, Principal of the Poona College of Science, and explained by him to the Council. The skilled artisans, he said, turned out by the school would be a benefit to the country and to the municipality alike.

THE additions to the Zoological Society's Gardens during the past week include an Indian Rhinoceros (*Rhinoceros unicornis* ♂) from India, presented by the Maharajah of Cooch Behar; a Tiger (*Felis tigris* ♂) from India, presented by the Zoological Gardens, Calcutta; a Chanting Hawk (*Melierax musicus*) from South Africa, a Red-throated Diver (*Colymbus septentrionalis*), European, presented by Lord Lilford, F.Z.S.; a Short-eared Owl (*Asio brachyotus*), British, presented by the Rev. Hubert D. Astley, F.Z.S.; ten Moorish Geckos (*Tarentola mauritanica*) from the borders of the Mediterranean, presented by Mr. J. C. Warburg; three Zebus (*Bos indicus* ♂ ♂ ♂) from India, a Montagu's Harrier (*Circus cineraceus*), European, deposited.

OUR ASTRONOMICAL COLUMN

THE SPECTROSCOPIC METHOD OF DETERMINING THE DISTANCE OF A DOUBLE STAR.—Mr. A. A. Rambaut, in a paper communicated to the Royal Irish Academy on May 24, discusses at some length the possibility of determining the distance of a double star by measures of the relative velocities of the components in the line of sight by means of the spectroscope. Of course, as soon as Dr. Huggins had demonstrated that it was practicable to measure the rate of approach or recession of a star, it was seen that it would be at least theoretically possible to determine the distance of a star by this method, but Mr. Rambaut does not merely repeat the suggestion, but examines the conditions of the problem that he may ascertain what chance there is of putting it into successful operation. His first step is to find the value of ΠV for the satellite star of any binary system, Π being the parallax in seconds of arc, and V the velocity of motion in the line of sight expressed in miles per second. The resulting formula is—

$$\Pi V = \frac{la^2\sqrt{1-\epsilon^2}\,.\,\sin(\phi-\lambda)\sin\gamma}{Pr\sqrt{1-\epsilon^2\cos^2\phi}} = k,$$

where ϕ denotes the angle between the tangent and major axis,
λ denotes the angle between the line of nodes and major axis,
γ denotes the angle between the plane of orbit and tangent plane to sphere,
P denotes period in years,
l denotes mean motion of earth in miles.

This equation therefore gives a relation between Π and V depending only on the period and the angular elements of the orbit, so that if either Π or V can be measured the other may at once be determined. If k be greater than unity, then either V must exceed ten miles per second, or Π one-tenth of a second of arc. If, then, the spectroscope show the lines in the spectra of both stars to be absolutely coincident, it follows that the parallax must exceed 0″·1, and the star will repay investigation. But if a measurable displacement be noticed, V can be determined, and the parallax will follow at once. So that "all double stars for which k is at any time greater than unity may be said to be within measurable distance either by the spectroscopic or the trigonometrical method." If, however, k be less than unity, the star may still chance to be within a measurable distance, for V may be small either from the small linear dimensions of the orbit or the length of the period; but if k be smaller than unity, and V be large, then we shall at once know, "with a certainty which the mere failure to measure its parallax trigonometrically could never reach, that the star is at an inconceivable distance from the solar system." Mr. Rambaut next proceeds to determine k for some 39 stars, the elements of whose orbits he takes for the most part from Houzeau's "Vade Mecum." In the case of five only does it exceed unity, viz., α Centauri 6·023, Sirius 5·400, 70*p* Ophiuchi 1·270, η Cassiopeiæ 1·247, and γ Coronæ Australis 1·224. Of these the parallax has already been determined for all but the last named. This star, the components of the pair being of nearly equal magnitude, would be well adapted for examination by the spectroscopic method if one of the new giant telescopes were employed, and since $k = 1·224$, had it been examined in 1880 either the velocity in the line of sight would have been found to exceed 12 miles per second, or the parallax to exceed 0″·1. Since a star fainter than the fifth and a half magnitude would be beyond the reach of even the most powerful instrument to successfully measure its movement in the line of sight, the field of inquiry is practically confined to α Centauri, and the following three stars for all of which k is fairly large though less than unity: ξ Ursæ Majoris 0·895, γ Virginis 0·624, and ζ Herculis 0·605. The result of Mr. Rambaut's inquiry is therefore to show that but little practical use can be made of the suggested combination of the two methods in the case of double stars.

NAMES OF MINOR PLANETS.—The following minor planets have recently received names:—No. 254, Augusta; No. 255, Oppavia; No. 257, Silesia; No. 260, Huberta; and No. 261, Prymno.

COMET FINLAY (1886 *e*).—Dr. J. Holetschek gives (*Ast. Nach.*, No. 2763) the following elements and ephemeris for this object, which, though now diminishing somewhat in brightness, becoming well placed for observation in northern latitudes:—

T = 1886 November 22·48418.

$$\left.\begin{aligned}\omega &= 315^\circ\ 21'\ 0''·5\\ \Omega &= 52\ \ 45\ \ 43·2\\ i &= 3\ \ \ 1\ \ \ 9·4\end{aligned}\right\}\text{ Mean Eq. 1886·0.}$$

$\log q = 9·997122$ $\log a = 0·533468$
$\log e = 9·850744$ Period = 6·31 years.

Ephemeris for Berlin Midnight

1887	R.A. h. m. s.	Decl. ° ′	log *r*	log Δ	Brightness
Jan. 0	23 49 17	1 2·3 S.	0·0565	9·9245	2·3
4	0 8 58	1 23·7 N.	0·0670	9·9343	2·1
8	0 28 10	3 45·0	0·0779	9·9461	1·9
12	0 46 52	5 59·9 N.	0·0889	9·9598	1·7

The brightness at the time of discovery is taken as unity.

COMET BARNARD (1886 *f*).—The following ephemeris for Berlin midnight is in continuation of that given in NATURE for December 9 (p. 134):—

1887	R.A. h. m. s.	Decl. ° ′	log *r*	log Δ	Brightness
Jan. 0	19 32 32	4 5·2 N.	9·8652	0·1478	10·5
5	19 53 52	1 13·5 N.	9·8935	0·1845	7·8
10	20 11 46	1 20·1 S.	9·9243	0·2177	5·8

The brightness at the time of discovery is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JANUARY 2-8

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 2

Sun rises, 8h. 8m.; souths, 12h. 4m. 14·9s.; sets, 16h. 1m.; decl. on meridian, 22° 55′ S.: Sidereal Time at Sunset, 22h. 49m.

Moon (at First Quarter) rises, 11h. 56m.; souths, 18h. 12m.; sets, 0h. 38m.*; decl. on meridian, 2° 25′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	6 41	10 39	14 37	22 38 S.
Venus	8 41	12 36	16 31	23 5 S.
Mars	9 34	13 52	18 10	19 35 S.
Jupiter	2 10	7 16	12 22	11 11 S.
Saturn	16 35*	0 40	8 45	21 49 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

Jan	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
4	μ Ceti	4	20 28	21 33	87 348
5	*f* Tauri	4	17 21	18 24	48 293
6	71 Tauri	6	18 50	19 7	359 334
6	θ¹ Tauri	4½	19 47	21 2	68 295
6	θ² Tauri	4½	19 54	20 55	47 316
6	B.A.C. 1391	5	21 2	22 19	108 289
6	85 Tauri	6	22 19	near approach	27 —
7	Aldebaran	1	0 17	1 15	165 283
7	111 Tauri	5½	19 51	near approach	340 —
7	115 Tauri	6	20 53	22 6	95 261

Jan.	h.	
2	20	Sun at least distance from the Earth.

Saturn, January 2.—Outer major axis of outer ring = 46″·4; outer minor axis of outer ring = 18″·5; southern surface visible.

Variable Stars

Star	R.A. h. m.	Decl. ° ′			h. m.	
U Cephei	0 52·3	81 16 N.	Jan.	2,	0 3	*m*
			,,	6,	23 43	*m*
λ Tauri	3 54·4	12 10 N.	,,	5,	2 12	*m*
S Cancri	8 37·5	19 26 N.	,,	5,	1 40	*m*
U Hydræ	10 32·0	12 48 S.	,,	5,		*M*
R Crateris	10 55·0	17 43 S.	,,	6,		*m*
S Leonis	11 5·0	6 4 N.	,,	6,		*M*
W Virginis	13 20·2	2 48 S.	,,	2,	4 0	*M*
δ Libræ	14 54·9	8 4 S.	,,	3,	19 41	*m*
			,,	6,	3 33	*m*
U Coronæ	15 13·6	32 4 N.	,,	7,	2 14	*m*
U Ophiuchi	17 10·8	1 20 N.	,,	4,	4 18	*m*
		and at intervals of			20 8	
W Cygni	21 31·8	44 52 N.	Jan.	4,		*M*
δ Cephei	22 25·0	57 50 N.	,,	4,	20 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers

The principal shower of the week is that of the *Quadrantids*, maximum January 2, radiant R.A. 228°, Decl. 53° N. Other showers are as follows:—From the borders of Gemini and Cancer, R.A. 119°, Decl. 16° N.; near θ Ursæ Majoris, R.A. 140°, Decl. 57° N.; near ζ Bootis, R.A. 220°, Decl. 13° N.

NOTES FROM THE OTAGO UNIVERSITY MUSEUM

IX.—*On the Nomenclature of the Brain and its Cavities*

IN working at the brain of the lower Vertebrata, the inconvenience of the received terminology of the cerebral cavities became so manifest, that I adopted the plan[1] of distinguishing each cavity by the simple expedient of placing before the syllable *cœle* (κοῖλος, κοιλία) the prefix used for the corresponding division of the brain in the systematic nomenclature adopted in Quain's "Anatomy." Thus, the entire cavity of the mid-brain of fishes, for which the usual names "aqueduct of Sylvius" or "iter a tertio ad quartum ventriculum" were unsuitable, became the *mesocœle* or cavity of the mesencephalon, the "lateral ventricles," the *prosocœles* or cavities of the prosencephala, and so on. A similar but more thorough-going reform had been previously and independently proposed by Burt Wilder (*Science*, ii., 1881, pp. 122 and 133), and adopted in Wilder and Gage's "Anatomical Technology" (New York, 1882). Prof. Wilder was good enough to write to me on the subject, and, after some correspondence had passed between us, he published an article[2] giving a full account of the nomenclature he proposed to adopt, and stating that certain of his terms (*e.g. neurocœle* for the entire cavity of the cerebro-spinal axis, and *encephalocœle* for the entire system of brain-cavities) had been proposed by me.

The chief features in Wilder's nomenclature are the following:—

(*a*) The adoption of *diencephalon* in preference to the more cumbrous *thalamencephalon* as the systematic name of the 'tween-brain (*Zwischenhirn*). The former term is given as a synonym by Allen Thomson (Quain's "Anatomy," ninth edition, vol. ii. p. 828), and is used by Rolleston in "Forms of Animal Life." In correspondence with this, the third ventricle becomes the *diacœle* (*thalamocœle*, mihi).

(*b*) The adoption of Quain's name of *epencephalon* for the cerebellum (*Hinterhirn*), and of *metencephalon* for the medulla oblongata (*Nachhirn*). Huxley's term, *myelencephalon*, for the latter division, is rendered inconvenient, to say the least, from the fact that it is used by Owen to designate the entire cerebro-spinal axis. The fourth ventricle is called the *metacœle* (*myelocœle*, mihi), and the cerebellar ventricle the *epicœle* (*metacœle*, mihi).

(*c*) The word prosencephalon is used in the same sense as by Quain, *i.e.* as including the whole of the fore-brain proper (*Vorderhirn*). Both Owen and Huxley, on the other hand, use this term as synonymous with cerebral hemisphere, *i.e.* speak of paired prosencephala. In correspondence with this, the entire cavity of the fore-brain is called the *prosocœle*, and the lateral ventricles themselves *procœles* (*prosocœles*, mihi).

(*d*) The unpaired cerebral rudiment of the embryo is distinguished as the *protocerebrum*.

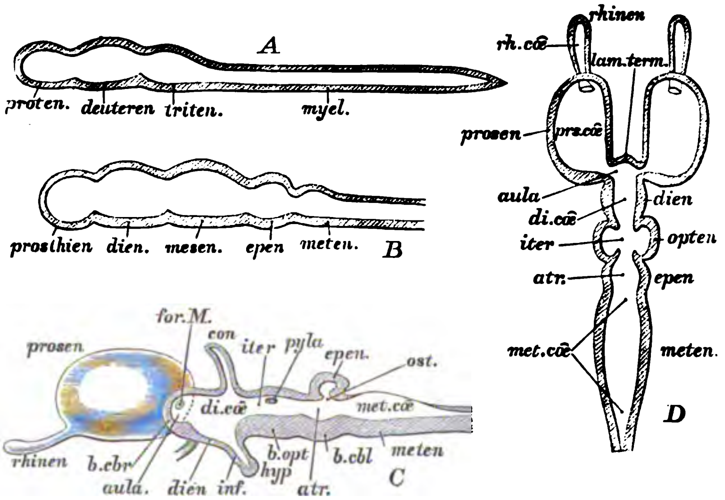

FIG. 1.—Diagrams showing three chief stages in the development of the nervous system. A, the *neuron* is divided into *myelon* and *encephalon*, the latter being again divided into the three primary vesicles, *protencephalon*, *deuterencephalon*, and *tritencephalon*. Similarly, the *neurocœle*, or general cavity of the neuron, is divided into *myelocœle* and *encephalocœle*, and the latter, again, into *proto-*, *deutero-*, and *trito-cœles*. B, the brain now consists of five encephalomeres, the *prosthi-*, *di-*, *mes-*, *ep-*, and *met-encephala*, containing respectively the *prosthio-*, *dia-* *meso-*, *epi-*, and *meta-cœles*. C, D, the brain has assumed its permanent form, and is shown in C in vertical, in D in horizontal section The prosthiencephalon has sent out paired prosencephala, a small unpaired portion, the *basicerebrum* (*b.cbr*), being left, the latter being bounded in front by the lamina terminalis (*lam.term*). Each prosencephalon has further given rise to a *rhinencephalon*. Similarly, the prosthiocœle now consists of unpaired *aulæ* and of paired *proso* and *rhino-cœles* (*prs.cœ*, *rh.cœ*), the former communicating with the aula by the foramina of Monro, or portæ (*for.M.*). The diacœle is continued above into the conarium (*con*), below into the infundibulum (*inf*) with the hypophysis (*hyp*). The mesencephalon consists of an unpaired ventral *basi-opticus* (*b.opt*), and of paired *optencephala*: its cavity of a median *iter*, and of paired *optocœles*, which communicate with the iter by the *pylæ*. The epencephalon is divided into a dorsal portion, the cerebellum, or epencephalon proper, and a ventral division, the *basi-cerebellum* (*b.cbl*), containing a cavity, the *atrium* (*atr*), which communicates with the epicœle proper, or cavity of the cerebellum, by the *ostium* (*ost*). The metencephalon and metacœle (*met.cœ*) have undergone but little alteration.

(*e*) The unpaired portion of the fore-brain, left by the budding-off of the cerebral hemispheres, is not specially named, but its cavity is termed the *aula*. This is a large and distinct cavity in some sharks (*e.g. Scymnus*, Fig. 2), but in the higher forms becomes the Y-shaped passage between the third and the lateral ventricles. This passage is sometimes spoken of as the "foramen of Monro," but the latter term is more correctly applied to the aperture between each of its anterior limbs and the corresponding lateral ventricle: this aperture Wilder calls the *porta*.

(*f*) The specialised cavities in the optic lobes of Amphibia and Sauropsida are called *optocœles*, the name *iter* (abbreviation of "iter a tertio ad quartum ventriculum") being retained for the unpaired portion of the *mesocœle* or entire cavity of the mid-brain. The iter communicates with each optocœle by a small aperture, the *pyla*. No name is given to the ventral portion of the mesencephalon after formation of the optic lobes, *i.e.* the part usually known by the awkward plural designation *crura cerebri*.

(*g*) The ventral portion of the epencephalon, the fibres of which become the pons Varolii in mammals, is called the *præ-oblongata*, the word *post-oblongata* being used as a synonym for metencephalon or medulla oblongata in the restricted sense.

(*h*) The entire cerebro-spinal axis is called the *neuron*, its cavity the *neurocœle*.

(*i*) The name *encephalocœle* is applied to the entire system of brain-cavities, or to the single cavity of the undivided embryonic encephalon.

(*k*) The name *myelocœle* is applied to the central canal of the myelon. This cavity is also distinguished into a lumbar dilatation, the *rhombocœle* (sinus rhomboidalis) and a contracted portion, the *syringocœle*.

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[1] "Notes on the Anatomy and Embryology of *Scymnus lichia*," *Trans. N.Z. Inst.* xv. (1882), p. 222; "A Course of Instruction in Zootomy," London, 1884.

[2] "Encephalic Nomenclature," *New York Medical Journal*, xli. (1885), pp. 325 and 354.

This scheme I propose to modify in certain particulars. For the sake of clearness I give diagrams (Fig. 1) showing three important stages in the development of the brain, as well as drawings of the brain of *Scymnus lichia* (Fig. 2). The latter shows with such diagrammatic clearness the typical structure of the Vertebrate encephalon that I now always use it as a starting-point for the study of that organ in my lectures.

(*a*) I propose to follow Wilder in his use of the words neuron and neurocœle, encephalon and encephalocœle, myelon and myelocœle (Fig. 1, A). The words syringocœle and rhombocœle appear to me unnecessary: I prefer to say that in some Vertebrates (*e.g.* birds) the lumbar region of the myelocœle is dilated into a sinus rhomboidalis.

(*b*) The three primary cerebral vesicles may be called respectively the *protencephalon*, *deuterencephalon*, and *tritencephalon;* their cavities the *protocœle*, *deuterocœle*, and (primary) *tritocœle* (Fig. 1, A).

(*c*) In what may be called the sub-primary stage of segmentation, the anterior, or first, and the posterior, or third, cerebral vesicles have each divided into two parts, the brain thus consisting of five encephalomeres, which I propose to call respectively the *prosthiencephalon* (= prosencephalon of Quain, *Vorderhirn*) and *diencephalon* (= thalamencephalon), derived from the protencephalon; *mesencephalon* (identical with the deuterencephalon), *epencephalon*, and *metencephalon*, formed by the constriction of the primitive tritencephalon. The cavities of these five brain-segments will be the *prosthio-*, *dia-*, *meso-*, *epi-*, and *meta-cœles* (Fig. 1, B).

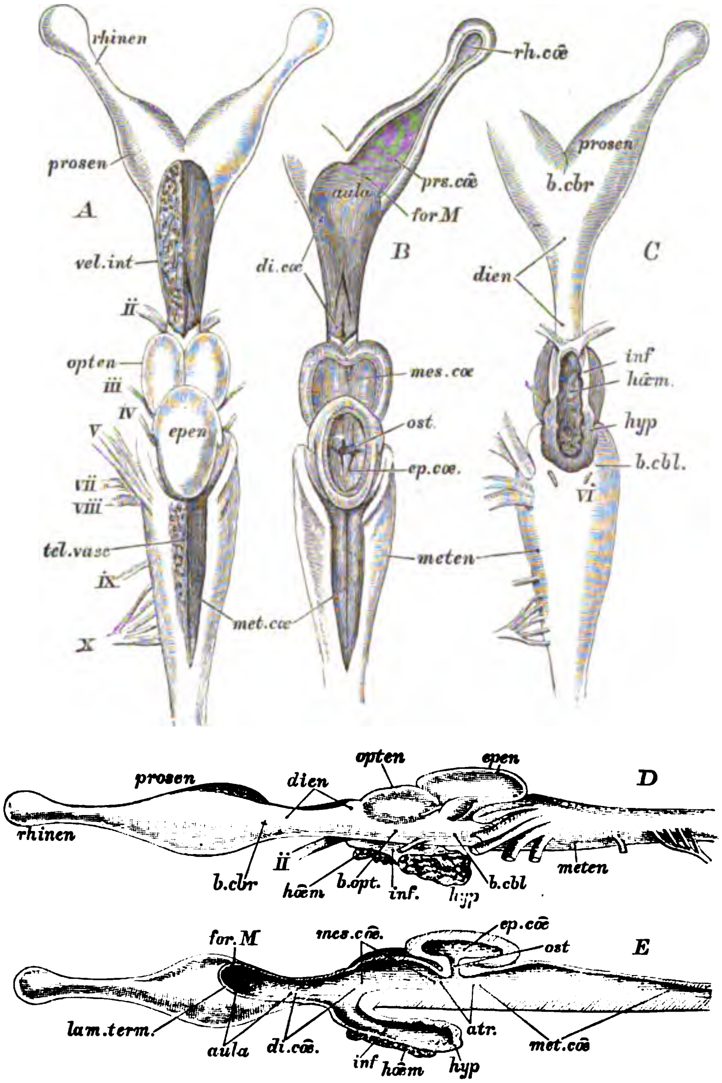

Fig. 2.—Five views of the brain of *Scymnus lichia* (nat. size). A, dorsal view of the brain, entire, save for the removal of the velum interpositum (*vel.int*) and tela vasculosa (*tel.vasc*) on the right side. B, the same, with the cavities opened from above. C, the entire brain from below. D, the entire brain from the left side. E, longitudinal vertical section. The letters have the same significance as in Fig. 1, except *hæm*, hæmatosac (saccus vasculosus); *vel.int*, velum interpositum; *tel.vasc*, tela vasculosa; and *ii–x*, cerebral nerves.

(*d*) In the next stage of differentiation of the fore-brain, the prosthiencephalon gives rise dorsally to the two cerebral hemispheres: I propose to follow Owen and Huxley in calling them the *prosencephala* (Fig. 1, C and D; Fig. 2, A—E, *prosen*); their cavities, or lateral ventricles, being named *prosocœles* (*prs.cœ*). The median portion of the prosthiencephalon, after separation of the prosencephala, may be called the *basicerebrum* (*b.cbr.*); its cavity, the *aula*, is Y-shaped, communicating by its unpaired posterior limb with the diacœle, by its paired anterior limbs with the prosocœles through the *foramina of Monro* (*for.M*), or *portæ*. The two prosencephala may be spoken of collectively by the old name, *cerebrum*, which, as Pye-Smith remarks,[1] "ought to be strictly limited to the hemispheres with the corpus callosum, corpora striata, and fornix."

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[1] "Suggestions on some Points of Anatomical Nomenclature" *Journ. of Anat. and Phys.*, xii. (1878), p. 154.

From each prosencephalon is budded off an olfactory lobe or *rhinencephalon*, containing a cavity, the *rhinocœle*, and usually divisible into a stem-like portion, or *crus*, and a dilated extremity, or *bulb*.

(*e*) The mesencephalon becomes differentiated dorsally into the paired optic lobes, or *optencephala*, the cavities of which, or *optocœles*, are frequently clearly distinguishable from the remaining median portion of the mesocœle, or *iter*, with which they communicate by small apertures, the *pylæ*. In many fishes, however, although the optic lobes are well formed, the mesocœle shows no distinction into iter and optocœles (cf. Fig 2, B and E). The unpaired ventral portion of the mid-brain, which in the lower forms shows no differentiation into crura cerebi may be distinguished as the *basi-opticus*.[1]

(*f*) In the epencephalon the dorsal region grows out into the cerebellum, or epencephalon proper, its ventral region, or *basi-cerebellum* (præ-oblongata, Wilder), being usually quite indistinguishable from the metencephalon, but becoming marked off in the Mammalia by the development of the pons. The anterior portion of the fourth ventricle of the adult, *i.e.* the portion corresponding to the basi-cerebellum, is of course epencephalic and not metencephalic, and may be distinguished as the *atrium* (Fig. 1, C and D; Fig. 2, E): it communicates, in Selachians, with the epicœle proper, or cerebellar ventricle, by a small aperture, the *ostium* (Fig. 2, B and E, *ost.*).[2]

It will be noticed that a mixture of Latin and Greek names occurs in the above scheme. This has been adopted so as to interfere as little as possible with the names in common use and with those proposed by Wilder. The Latin names, moreover, are introduced with a certain consistency; *e.g.* the basi-cerebrum is the median unpaired portion of the protencephalon, the basi-opticus of the mesencephalon, and the basi-cerebellum of the epencephalon; similarly, the cavities of these basal regions are respectively the aula, the iter, and the atrium, the main "ventricles" being all distinguished by names of Greek origin.

The advantages claimed for the proposed nomenclature are the following:—

(*a*) Names are given to important structures which have hitherto been designated by more or less lengthy phrases, *e.g.* basi-cerebrum, aula, mesocœle, &c

(*b*) The systematic brain-nomenclature of Quain is brought up to date by introducing changes rendered desirable by the progress of animal morphology.

(*c*) The cavities of the brain are distinguished by systematic names which have an evident connection with those of the encephalomeres themselves, instead of by names which have no relation either with the regions of the brain in which the cavities occur, or with one another.

(*d*) The description of the nervous system of the lower Chordata is simplified. For instance, in *Amphioxus*, one may say that the neuron shows no distinction externally into encephalon and myelon, but that the neurocœle is dilated anteriorly into a small encephalocœle.

While agreeing with Prof. Wilder in the advisability of making the changes proposed above in the nomenclature of the central nervous system, I differ from him in failing to see the necessity, or, indeed, the desirability, of making all binomial names monomial. Such names, for instance, as anterior commissure, corpus callosum, lamina terminalis, which are not misleading, and which do not require to be connected with homologous parts by a consistent nomenclature, may very well be left alone; although, if one could start *ab initio*, I quite admit that the substitutes proposed by Wilder might be preferable. In any case, however, his name *pseudocœle* is thoroughly deserving of adoption as a substitute for the misleading appellation, "fifth ventricle."

Prof. Wilder's suggestion that *encephalon* should be Anglicised into *encephal* is worthy of consideration, especially as the word ought to be written *encephalos*, and it would be an advantage to get rid of the incorrect neuter termination. I have adopted the abbreviated form in the following table, which shows at a glance the nature of the proposed scheme of nomenclature. The names of the various divisions of the nervous system are printed in capitals, those of the corresponding cavities in italics.

- NEURON *Neurocœle*
  - ENCEPHAL *Encephalocœle*
    - PROTENCEPHAL *Protocœle*
      - PROSTHIENCEPHAL *Prosthiocœle*
        - RHINENCEPHALS *Rhinocœles*
        - PROSENCEPHALS (CEREBRUM) *Prosocœles*
      - DIENCEPHAL *Diacœle*
        - BASI-CEREBRUM *Aula*
    - DEUTERENCEPHAL *Deuterocœle*
      - MESENCEPHAL *Mesocœle*
        - OPTENCEPHALS (OPTIC LOBES) *Optocœles*
        - BASI-OPTICUS *Iter*
    - TRITENCEPHAL *Tritocœle*
      - EPENCEPHAL *Epicœle*
        - EPENCEPHAL (CEREBELLUM) *Epicœle* (definitive)
        - BASI-CEREBELLUM *Atrium*
      - METENCEPHAL *Metacœle*
  - MYELON *Myelocœle*

My object in writing this note is not so much to get my own or any other system of names adopted, as to urge the necessity for a reform in the nomenclature of the central nervous system and its cavities—a necessity which no comparative anatomist, especially if he be a teacher, can fail to see. Recent investigations of the skull, shoulder-girdle, urinogenital organs, &c., of Vertebrates have necessitated corresponding changes in nomenclature, and similar changes are constantly being made among the various Invertebrate groups. It would certainly be a great boon, both to teachers and students, if a like reform could be generally adopted for the Vertebrate nervous system.

Dunedin, N.Z., September 15 T. JEFFERY PARKER

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[1] "Critics will no doubt object to using an adjective as a substantive, but how far this is admissible is entirely a matter of usage," &c. (Pye-Smith, *loc. cit.*, p. 174, note).

[2] *Cerebellum* is one of the few names in the older brain-nomenclature which presents no ambiguity, so that the only reason for giving it a Greek synonym is the logical satisfaction of having a similar set of names for all the great divisions of the brain. Strictly speaking, the word epencephalon, being synonymous with *cerebellar segment*, ought not to be used for the cerebellum itself, and *hyperencephalon* might be used instead, with *hypercœle* for cerebellar ventricle.

THE IMPERIAL INSTITUTE

THE following is the report of the Committee appointed by H.R.H. the Prince of Wales to prepare a scheme for the proposed Imperial Institute:—

The committee appointed by your Royal Highness to frame a scheme for an Imperial Institute intended to commemorate the fiftieth year of Her Majesty's reign beg leave to submit to your Royal Highness the following report.

They do not fail to remember that the scheme which your Royal Highness indicated in your letter of September 13 last to the Lord Mayor of London had its origin in the remarkable interest excited by the recent Exhibition, by which not only the material products, resources, and manufactures, but the loyal feeling of the great colonies and possessions of Her Majesty's Empire, were illustrated in a most signal manner.

The object, therefore, which naturally suggested itself first to the committee was the development, with some necessary modifications, of your Royal Highness's idea of creating a permanent representation of the resources and progress of the colonies and India.

On pursuing, however, the consideration of the subject, the committee became persuaded that a memorial really worthy of the jubilee year of Her Majesty's reign could not be confined in its objects to any one part or parts of Her Majesty's Empire, and that it must in some form and degree also comprehend a representation of the United Kingdom.

Their desire, therefore, in the following outline of the scheme which they recommend is to combine in a harmonious form, and with a view to some practical and useful purpose, a representation of the colonies and India on the one hand and of the United Kingdom on the other.

They submit that this object will be best indicated by giving to the memorial the title of The Imperial Institute of the United Kingdom, the Colonies, and India.

They think that the Institute should find its home in buildings of such a character as worthily to commemorate the jubilee year of the Queen's reign, and to afford accommodation suitable for an institution combining the important objects which they now proceed to describe.

It is obvious that several departments of the Institute, such as the hall, conference rooms, &c., which will be found described under the Colonial and Indian Section and the United Kingdom Section respectively, will be common both to the colonies and India and to the United Kingdom: but as others have special relation to a particular portion of Her Majesty's dominions, it will be found convenient to make the following division.

A. *Colonial and Indian Section.*—The object of the Colonial and Indian Section will be to illustrate the great commercial and industrial resources of the colonies and India, and to spread a knowledge of their progress and social condition.

To this end provision should be made for—

(1). The display in an adequate manner of the best natural and manufactured products of the colonies and India, and in connection with this the circulation of typical collections throughout the United Kingdom.

(2). A hall for the discussion of colonial and Indian subjects, and for receptions connected with the colonies and India.

(3). The formation of colonial and Indian libraries, and establishing in connection therewith reading, news, and intelligence rooms.

(4). The incorporation in some form into the proposed Institute of the Royal Colonial and Royal Asiatic Society, if, as is hoped, it be possible to bring about such a union.

(5). The collection and diffusion of the fullest information in regard to the industrial and material condition of the colonies, so as to enable intending emigrants to acquire all requisite knowledge. Such information might be advantageously supplemented by simple and practical instruction. An emigration office of this character should be in correspondence with the provincial towns, either through the free libraries or by other means, so that information may be readily accessible to the people. These objects would be greatly facilitated if, as may be hoped, the Government should consent to the transfer to the buildings of the Institute of the recently formed Emigration Department, which would, by a close connection with the Institute, largely increase its usefulness.

Facilities might be afforded for the exhibition of works of colonial and Indian art.

It is also considered desirable that means should be provided, not for a general exhibition, but for occasional special exhibitions of colonial and Indian produce and manufactures. At one time a particular colony or portion of the Empire may desire to show its progress; at another time a general comparison of particular industries may be useful. Whilst the permanent galleries would exhibit the usual commercial or industrial products of the several colonies and India, the occasional exhibitions would stimulate and enlist the sympathies of colonial and Indian producers, and keep up an active co-operation with the industrial classes of this country.

B. *United Kingdom Section.*—The leading objects of this Section will be to exhibit the development during Her Majesty's reign and the present condition of the natural and manufactured products of the United Kingdom, and to afford such stimulus and knowledge as will lead to still further development, and thus increase the industrial prosperity of the country.

We submit that these objects may be carried out by making provision for the following purposes:—

(1). Comprehensive collections of the natural products of the United Kingdom, and of such products of other nations as are employed in its industries, with full scientific, practical, and commercial information relating thereto.

(2). Illustrations of manufactured products, typical of their development and present condition, of trades and handicrafts, and their progress during the Queen's reign, including illustrations of foreign work when necessary for comparison; together with models illustrating naval architecture, engineering, mining, and architectural works.

(3). A library for industrial, commercial, and economic study, which should contain standard works and reports on all subjects of trade and commerce. It will be desirable also to include a library of inventions of the Empire, and, as far as possible, of the United States and other countries.

(4). Reading and conference rooms supplied with English, colonial, and foreign commercial and technical periodicals, and a fully-equipped map room for geographical and geological reference. The conference rooms would be of value for meetings of Chambers of Commerce and other bodies of a kindred nature.

(5). The promotion, in affiliation with the Imperial Institute, of commercial museums in the City of London and in the commercial centres of the provinces. To these the Institute would contribute specimens, samples, and exhibits of the commercial products likely to be specially valuable in particular localities. There should also be an organisation to connect the Imperial Institute with the provincial centres by lectures, conferences, the circulation of specimens, and other means.

It is hoped that the Institute may lead to the organisation of high schools of commerce, such as are now established in the chief commercial towns of most Continental countries, but which have, as yet, unfortunately no existence in the United Kingdom.

(6). The building will also advantageously afford accommodation for (*a*) comparing and examining samples by the resources of modern science, and (*b*) the examination of artisans under the various schemes already existing for the promotion of technical education.

Space should also be provided for occasional exhibitions of separate industries, or of the special industries carried on in great provincial centres: for example, there might at one time be an exhibition of iron manufactures, at another of pottery, at another of textile fabrics, &c., which would tend to stimulate improvement in the different departments of industrial life. This object might be assisted by separate exhibitions of the handiwork of artisans.

The committee, having detailed the general nature of their suggestions under these heads, desire to add that they do not anticipate the exhibits in the collections remaining unchanged. They contemplate that as improvements are made from time to time the later and better results would displace those out of date.

They have had to consider how the space should be distributed between the United Kingdom on the one hand and the colonies and India on the other, and they recommend that whatever portion of the buildings is not required for purposes manifestly common to both should be allotted to the two sections fairly in equal parts.

C. *Government of Institute.*—The committee recommend that a new body, entirely independent of any existing organisation, should be created for the government of the Institute. This body should be thoroughly representative of the great commercial and industrial interests of the Empire. The colonies and India should have a fair share in the government of the Institute, and each colony should have special charge of its own particular department, subject, of course, to the general management of the entire institution.

The method of carrying this out would be prescribed by the Charter, after full consideration by Her Majesty in Council.

D. *Site.*—The committee, being fully conscious of the advantage of a central position for the Institute, have considered the various possible sites, and have, as far as has been within their power, obtained estimates of their cost.

To carry out the several objects which the committee have indicated, a large space is necessary. The committee have been unable to find any such suitable site in the central parts of London, except at a cost which, looking at the probable amount of subscriptions, would, after the purchase of the ground, leave a sum wholly inadequate for the erection and maintenance of the buildings, and for carrying out the objects of the Institute.

The site of about five acres recently secured for the New Admiralty and War Offices is valued at 820,000*l.*, or rather over 160,000*l.* per acre. That now vacant in Charles Street, opposite the India Office, is less than an acre, and would cost at least 125,000*l.*; probably another acre might be secured by private contract, so that the value of a limited site in this position would not be less than 250,000*l.* It has been suggested that a single acre not far from Charing Cross might be obtained for 224,000*l.* Two and a half acres on the Thames Embankment have been offered for 400,000*l.*; and it is stated that six acres might be procured from Christ's Hospital at 600,000*l.* Another good central position has been suggested, consisting of two and a half acres, which has been valued at 668,000*l.*

It is, of course, probable that these sites might be obtained at somewhat less than the prices asked, but, allowing for this, it is obvious that the purchase of any adequate area would involve the expenditure of a quarter to half a million.

The committee have therefore been forced to abandon the hope of obtaining a central site within the limits allowed by any probable subscription.

The attention of the committee was then drawn to the property at South Kensington belonging to the Commissioners for the Exhibition of 1851. This property was bought out of the profits of that Exhibition, with the express object of offering sites for any large public buildings which might be required for the promotion of science and art.

Under these circumstances, the committee submit to your Royal Highness that the Imperial Institute may well establish a claim for the grant of a site of sufficient magnitude on property bought and reserved for public institutions of this character.

Though sensible of the objections that may be urged against the situation at South Kensington, the committee think that the advantage must be obvious of obtaining a sufficient site virtually free of cost, so that the whole of the subscriptions may be devoted to providing a building for and establishing and maintaining the Institute.

The committee, while guided in the recommendation of a site by the considerations they have indicated, think it right to add that there are some incidental advantages connected with that at South Kensington.

In that locality are combined the City and Guilds Technical College, the Royal College of Music, and the Government Museums and Schools of Science and Art, which ought to be in immediate proximity to an Imperial Institute of the character which we have described.

The technical character of the collections and exhibitions of the Imperial Institute has a natural connection with the collections of science and art in the Government Museums.

E. *General Observations.*—An Imperial Institute for the United Kingdom, the Colonies, and India, would fail in its chief object if it did not constantly keep in view that it ought to be a centre for diffusing and extending knowledge in relation to the industrial resources and commerce of the Empire.

The necessity for technical education is now fully appreciated, because the competition of industry has become, in a great measure, a competition of trained intelligence. The committee, however, do not recommend that the Imperial Institute should aspire to be a college for technical education. Many of the large towns in Great Britain have recently established colleges or schools of science and art. The Imperial Institute might serve to promote technical education in these, and to unite them with colleges of larger resources which have been founded or formed branches for the purpose in the metropolis. It is too much to hope that an active co-operation of this character between the provincial centres and London could be at once undertaken by the Imperial Institute. But the committee bear in mind that, in their last report, the Commissioners of 1851 have indicated an intention to assist in carrying out such a scheme. If the Commissioners would contribute three or four thousand pounds annually, it would be possible to establish scholarships which might enable promising candidates of the working classes to attend the local institutions, and even, when it was desired, to complete their technical education in colleges of the metropolis. In addition to this aid, the Imperial Institute might be able, in other ways, to promote the foundation of scholarships both in connection with the colonies and provincial centres, in the hope of still further extending these benefits to the working classes.

In conclusion, the committee submit that an Imperial Institute such as they have sketched in broad outline would form a fitting memorial of the coming year, when Her Majesty the Sovereign of this Empire will celebrate the jubilee of her happy reign. It would be an emblem of the unity of the Empire, embracing as it does all parts of the Queen's dominions, and tending to promote that closer union between them which has become more and more desired. It would exhibit the vast area, the varied resources, and the marvellous growth, during Her Majesty's reign, of the British Empire. It would unite in a single representative act the whole of her people; and, since both the purpose and the effect of the Institute will be to advance the industrial and commercial resources of every part of the Empire, the committee entertain a confident hope that Her Majesty's subjects, without distinction of class or race, will rejoice to take part in offering this tribute of love and loyalty. —HERSCHELL (Chairman), CARNARVON, REVELSTOKE, ROTHSCHILD, G. J. GOSCHEN, LYON PLAYFAIR, HENRY JAMES, HENRY T. HOLLAND, H. H. FOWLER, C. T. RITCHIE, FRED. LEIGHTON (President of the Royal Academy), ASHLEY EDEN, OWEN T. BURNE, REGINALD HANSON (Lord Mayor), J. PATTISON CURRIE (Governor of the Bank of England), JOHN STAPLES, FREDERICK ABEL (Vice-President of the Society of Arts), J. H. TRITTON (Chairman of the London Chamber of Commerce), NEVILE LUBBOCK, HENRY BROADHURST.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the annual election to scholarships and exhibitions in St. John's College, for candidates who have not yet commenced residence, the following awards were made:—

Foundation Scholarships: (80*l.*) to A. Vaughan, University College School, and H. Reeves, Surrey County School; (50*l.*) to O. W. Owen, Liverpool Institute (all for Mathematics); (60*l.*) to J. T. Hewitt, South Kensington School of Science, for Chemistry.

Minor Scholarships: (50*l.*) to G. T. Bennett, University College School, and W. J. Dobbs, Wolverhampton School, for Mathematics, and to R. A. Lehfeldt, for Physics.

Exhibitions to J. J. Alexander, Queen's College, Belfast, for Mathematics, and to F. F. Blackman, St. Bartholomew's Hospital, for Physiology and Botany.

SCIENTIFIC SERIALS

THE articles in the *Journal of Botany* for November and December are mostly descriptive. Mr. H. N. Ridley concludes his description of the Monocotyledonous plants collected in New Guinea by Mr. Forbes, including a number of new species; Mr. J. G. Baker, his synopsis of the Rhizocarpeæ, with a monograph of *Pilularia;* and Dr. Trimen, his valuable account of the flora of Ceylon and its relations to the climate of the island.—Mr. J. G. Baker describes some new species of Liliaceæ from the Cape of Good Hope.—The other original papers refer to the distribution of British plants.

Nuovo Giornale Botanico Italiano for October.—G. Venturi describes several species of moss new to the Italian flora, or rare or critical species.—L. Macchiati, on the extra-floral nectaries of the Amygdaleæ, describes nectariferous glands on the leaf-stalk of *Persica vulgaris*, *Cerasus vulgaris*, *Prunus domestica*, and *Amygdalus communis.* These agree in function with the extra-floral nectaries in other European plants, in serving as a protection against the attacks of caterpillars; while in the case of natives of Tropical America, their purpose is invariably to protect against the attacks of the ant *Oecodoma*, by attracting other ants, enemies to this species. The author records a diurnal periodicity in the amount of nectar exuded from the glands, which reaches its maximum early in the morning, its minimum in the afternoon.—B. Scortechini describes several new species of Scitamineæ from the Malayan peninsula, including a new genus, *Lowia*.—Prof. T. Caruel has a note on the fruit and seeds of the cacao.—P. Severino describes the variety *purpurea* of *Aceras anthropophora*, and the micro-chemical reactions of the purple cells.—Two teratological papers complete the list: on viviparity and prolification in *Spilanthes caulirrhiza*, by Dr. F. Tassi; and teratological notes (on *Aegle sepiaria*, *Lysimachia ephemerum*, and *Saxifraga crassifolia*), by C. Massalongo.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 16.—"On a Varying Cylindrical Lens." By Tempest Anderson, M.D., B.Sc. Communicated by Prof. A. W. Williamson.

The author has had constructed a cylindrical lens in which the axis remains constant in direction and amount of refraction, while the refraction in the meridian at right angles to this varies continuously.

A cone may be regarded as a succession of cylinders of different diameters graduating into one another by exceedingly small steps, so that if a short enough portion be considered, its curvature at any point may be regarded as cylindrical. A lens with one side plane and the other ground on a conical tool is therefore a concave cylindrical lens varying in concavity at different parts according to the diameter of the cone at the corresponding part. Two such lenses mounted with axes parallel and with curvatures varying in opposite directions produce a compound cylindrical lens, whose refraction in the direction of the axes is zero, and whose refraction in the meridian at right angles to this is at any point the sum of the refractions of the two lenses. This sum is nearly constant for a considerable distance along the axis so long as the same position of the lenses is maintained. If the lenses be slid one over the other in the direction of their axes, this sum changes, and we have a varying cylindrical lens. The lens is graduated by marking on the frame the relative position of the lenses when cylindrical lenses of known power are neutralised.

Lenses were exhibited varying from 0 to − 6DCy, and from 0 to + 6DCy.

Linnean Society, December 16.—W. Carruthers, F.R.S., President, in the chair.—H.R.H. the Prince of Wales was elected an Honorary Member of the Society.—Messrs. A. Bawtree, F. Justen, T. N. Mukharji, F. W. Oliver, and R. V. Sherring were elected Fellows, and G. Nicholson an Associate, of the Society.—The President announced that Sir George MacLeay, K.C.M.G., had presented to the Society a portrait of the late Rev. W. Kirby, the distinguished entomologist, and the manuscripts and correspondence of his father, Alexander MacLeay (elected F.L.S. 1794), formerly Secretary to the Society. For these acceptable donations, a special vote of thanks was accorded by the Fellows.—Prof. F. O. Bower exhibited a series of photographs illustrating the vegetation of Ceylon.—Mr. E. A. Heath showed a stormy petrel, *Procellaria pelagica*, which was picked up alive in Kensington Gardens on December 9; the bird evidently having been driven inland by the great storm of the previous day.—Mr. D. Morris drew attention to the fresh leaves, and the fibres extracted therefrom, of *Agave salmdyckia* and *A. Ixtli*.—Mr. W. T. Thiselton Dyer showed one of the volumes of "Honzo Zufu" ("Illustrations and Brief Descriptions of the Plants of Japan"), by Iwasatti Tsanemasa, which consists of ninety-six volumes containing 2000 coloured figures. Only two or three copies of this important botanical work are known to be complete, as a great part of it only exists in the original native hand-work.—The President exhibited a spike of maize from an ancient Peruvian grave, also samples of prehistoric wheat from ancient British and Romano-British burial-mounds in Wiltshire.—Mr. G. J. Romanes read a paper on the sense of smell in dogs, a report of which we hope to give in a future number.—Mr. C. T. Druery gave a communication on a new instance of apospory in *Polystichum angulare*, var. *pulcherrimum*. He infers that the formation of the prothallus is preceded by a very different series of phenomena from those already recorded. In the one case the prothalli are simple extensions of the cellular substance of the tips of the pinnules commencing at points quite beyond the venation, and produce no root-hairs unless brought into contact with the soil. In the other case, however, the prothallus is a direct outgrowth of the tip of a veinlet, and at once produces root-hairs in abundance long before it assumes any other characteristic of a prothallus, and finally the resulting prothallus is much thicker in substance.—A paper was read on apospory and allied phenomena, by Prof. F. O. Bower. The term "sporal arrest" is applied to all cases where such spores do not come to functional maturity. The arrest is often, but not always, followed by substitutionary or correlative vegetative growths: these take the form of buds, similar to the sporophyte which produced them, and then would be termed cases of "sporophytic budding"; but in other cases the correlative growths may assume the characters of the oophyte or prothallus. Where this happens, the phenomenon is termed "apospory." This direct transition from the sporophyte to the oophyte was *induced* some ten years ago in certain mosses, by Pringsheim and Stahl; and it is now described in detail in two ferns, an *Athyrium* and a *Polystichum*. Both plants were found some years ago growing wild, and the fact of the transition was recognised by Mr. Druery and Mr. Wollaston, and has been already published by the Linnean Society. The present paper describes these and similar phenomena in detail, and shows how in the *Polystichum* at least four different modes of origin of the oophytes may be distinguished, two being in connection with the sorus, while two are at points apart from the sorus, and may even occur on fronds which bear no sori at all. The latter part of the paper is occupied by comparing these phenomena with others already known in higher and lower plants. The general conclusion is that the whole phenomenon of apospory is to be regarded rather as a sport than as a reversion bearing deep morphological conclusions with it.

Chemical Society, December 2.—W. Perkin, F.R.S., Vice-President, in the chair.—Mr. Forbes Rickard was formally admitted a Fellow of the Society.—The following papers were read:—Bismuthates, by M. M. Pattison Muir and Douglas J. Carnegie.—The action of inorganic compounds on living matter, by James Blake, M.D.—Morindin and morindon, by T. E. Thorpe, F.R.S., and T. H. Greenall.—The hydration of salts: cadmium chloride, by S. U. Pickering.—The decomposition of sodium carbonate on fusion, by S. U. Pickering.—Derivatives of tolylbenzene, by Thomas Carnelley, D.Sc. (Lond.), and Andrew Thomson, D.Sc. (Edin.).—The amount of chlorine in rain-water collected at Cirencester, by Edward Kinch, Royal Agricultural College, Cirencester.—Some analogous phosphates, arsenates, and vanadates, by John A. Hall, student in the Laboratory of Owens College.—Agricultural experiments with iron sulphate as a manure during 1886, by A. B. Griffiths, Ph.D.

Royal Meteorological Society, December 15.—Mr. W. Ellis, F.R.A.S., President, in the chair.—Mr. G. R. Farncombe, B.A., Mr. C. E. B. Hewitt, B.A., and Capt. S. Trott were elected Fellows of the Society.—The following papers were read:—On the proceedings of the International Congress of Hydrology and Climatology at Biarritz, by Mr. G. J. Symons, F.R.S. This Congress was held in October, and was divided into three sections, viz. Scientific Hydrology, Medical Hydrology, and Climatology, Scientific and Medical. The total number of papers read was 109. An Exhibition was also held in connection with the Congress. The excursions were of primary importance to the medical men, and extended over a period of three weeks. The places visited were: Bayonne, Cambo, Dax, Arcachon, Pau, Eaux-Bonnes, Eaux-Chaudes, Cauterets, Lourdes, Bagnères-de-Bigorre, Luchon, Ussat, Ax, Montpellier, Cette, Boulou, Amélie-les-Bains, La Preste, Banyuls-sur-Mer, and Thues.—Report on the phenological observations for 1886, by the Rev. T. A. Preston, M.A., F.R.Met.Soc. The weather was, on the whole, very ungenial and everything much retarded; it was also very fatal to insect life, so that the complaints on this head have been far less than usual. Bush fruits were *very* abundant; strawberries and peas were spoilt by drought in many places; stone fruits, except plums, were not abundant; plums were extraordinarily plentiful, so much so that they realised nothing in the markets, the cost of picking and carrying often being more than they realised; apples were very poor, from the destruction of the bloom by heavy rain. Hay was good and plentiful, and well harvested; corn and other grain were not up to an average: root-crops were, as a rule, remarkably good.—A criticism of certain points of Prof. Langley's researches on solar heat, by Prof. S. A. Hill, B.Sc., F.R.Met.Soc. These experiments were carried out at Mount Whitney, in Southern California, during 1881.—Account of the hurricane of March 3–4, 1886, over the Fiji Islands, by Mr. R. L. Holmes, F.R.Met.Soc. This storm was the most destructive that has ever been known to occur in the Fiji group. The lowest barometer reading was 27·54 inches at Vuna, in Taviuni. The storm was accompanied by a great wave from 18 to 30 feet in height, which swept over the land and caused an immense amount of damage. It was reported that fifty vessels were wrecked and sixty-four lives lost during this hurricane.—Results of meteorological observations made at the Military Cemetery, Scutari, Constantinople, 1866-85, by Mr. W. H. Lyne. The annual mean temperature is 58°·4; the highest temperature registered was 103°·6 on June 22, and the lowest 13°·0, on January 25, both in 1869. The annual rainfall is 29·29 inches; the greatest fall in one day was 4·06 inches on September 25, 1866.

Physical Society, December 11.—Prof. McLeod, Vice-President, in the chair.—W. Natanson, Ed. Natanson, the Hon. R. Abercromby, Jul. Verteimer, and H. M. Elder were elected Members of the Society.—The following papers were then read:—On the influence of change of condition from the liquid to the solid state on vapour-pressure, by Prof. W. Ramsay, Ph.D., and Sydney Young, D.Sc., read by Dr. Young. The authors refer to some experiments published in *Wiedemann's Annalen*, vol. xxviii. p. 400, by W. Fischer, on the above subject, which show that the vapour-pressure of ice and solid benzene are less than those of water and liquid benzene at the same temperatures. By using the formula $p = a + bt + ct^2$ to express the relation between the pressure and temperature of saturated vapours, Fischer arrives at the absurd result that the vapour-pressure of liquid benzene is not identical with that of solid benzene at melting-point. If the above formula be replaced by $\log p = a + bat$, it is shown that the anomaly disappears. The authors have measured the vapour-pressures of solid and liquid benzene by the dynamical method, and obtain results agreeing closely with those of Fischer determined statically. They also calculate the vapour-pressure of solid benzene from that of the liquid, using the formula—

$$P_{t-1} = P \quad - (P'_t - P'_{t-1})\left(\frac{V_{t-\frac{1}{2}} + F_{t-\frac{1}{2}}}{V_{t-\frac{1}{2}}}\right),$$

where P_t and P'_t are the vapour-pressures of the solid and liquid at temperature t, $V_{t-\frac{1}{2}}$ = heat of vaporisation of liquid, and $F_{t-\frac{1}{2}}$ = heat of fusion of solid at temperature $t - \frac{1}{2}$. The numbers so obtained are in accordance with those determined experimentally.—On the nature of liquids as shown by the thermal properties of stable and dissociable bodies, by the same authors, read by Prof. Ramsay. From experiments on the vapour-density and heat of vaporisation of stable and dissociable bodies, the authors arrive at two important results: (1) that for stable bodies, such as alcohol and ether, the density of their saturated vapours increases with rise of temperature, whereas for bodies such as acetic acid and nitric peroxide the vapour-density attains a minimum at a certain temperature, and increases with either rise or fall of temperature; (2) the heat of vaporisation of alcohol decreases with rise of temperature, but that of acetic acid attains a maximum at about 110° C., and decreases with rise or fall of temperature. From these results the authors seek to prove that the difference between stable liquids and their vapours consists in the relative proximity of the molecules, this proximity being greater in liquids than gases, and that the molecules of stable liquids are not more complex than those of their gases. Prof. Pickering dissented from this view, and thought that the molecules of liquids are aggregations or compounds of those of the gases. In answer to inquiries by the authors, Mr. Lewis Wright said that bodies which rotate the plane of polarisation of light when in the liquid state also rotate it in a proportionate degree when gaseous; and Capt. Abney remarked that stable liquids and their vapours give similar absorption-spectra, whereas those of dissociable bodies differ considerably. Both these facts seem to support the view put forward by the authors.—An account of Cauchy's theory of reflection and refraction of light, by Mr. James Walker, M.A. This paper is intended as a statement of the work previously done in the subject, and gives references to the original papers and "reproductions," &c., which will be of great value to persons studying this important branch of the theory of optics.—Mr. Shelford Bidwell exhibited and described a voltaic cell, in which the electrolyte is dry peroxide of lead. It consists of carefully dried peroxide placed between plates of lead and sodium, and gives a comparatively strong current, which passes from the sodium to the lead within the cell.

CAMBRIDGE

Philosophical Society, November 8.—Mr. Trotter, President, in the chair.—The following communications were made:—On the cœlum and body-cavity of Peripatus and the Arthropoda, by Mr. A. Sedgwick.—Note on the "vesicular vessels" of the onion, by S. H. Vines, M.A. Christ's, and A. B. Rendle, St. John's. In investigating the vesicular organs with the object of determining whether or not the transverse walls are perforated so as to place the cavities of successive segments in communication, the authors observed that, in the quiescent winter condition of the bulb, there are patches of callus—easily made conspicuous by staining with corallin—on the transverse walls. From this they infer that the transverse walls are perforated, the canals through them being open in the active, and closed by callus in the quiescent, condition of the bulb, just as is the case with sieve-tubes. This inference has, however, to be confirmed by an investigation of the bulb in the active condition. The authors also observed that each segment of a vesicular vessel contains a large nucleus.—On *Epiclemmydia lusitanica*, a new species of Alga, by Mr. M. C. Potter. During August and September, the author, with assistance from the Worts Travelling Scholars' Fund, investigated the life-history of a new species of Alga, now named *Epiclemmydia lusitanica*, which lives on the backs of the tortoises inhabiting the pools of Southern Europe. This Alga, which to the naked eye appears as small green roundish patches, is found to consist of a number of cells closely applied to tortoise-shell, but which are only a few layers deep, here and there penetrating into the tortoise-shell and causing it to flake off. The cells next to the tortoise-shell always force their way into any available crack, where they divide, and thus penetrate to some depth into the shell of the tortoise, and finally cause it to be flaked off. The Alga is reproduced by means of zoospores formed in the external layer of cells. These zoospores are all exactly similar, and swim about for a considerable time, after which they come to rest and germinate.—On a peculiar organ of *Hodgsonia heteroclita*, by Mr. Walter Gardiner. The author gave some account of the gland-bearing organs which are found in *Hodgsonia*—one in the axil of each of the foliage leaves. A study of the development of these organs demonstrates that they are peculiarly modified leaves, or rather bracts, since they are associated with the rudimentary flower-bud. They are doubtless identical with the similarly modified bracts which occur in connection with the fully developed flowers. The glands are found on the lower surface of the bract, and belong to the same type as those of *Luffa*, although of a distinctly higher order. Glands of a similar nature also occur on the under surface of the foliage leaves and on the sepals. The substance secreted by the glands is most probably of the nature of nectar, and the whole structures are to be regarded as extra floral nectaries. Having shortly described their histology, the author proceeded to make some remarks upon their function. A careful survey of the various gland-bearing genera of the *Cucurbitaceæ* and *Passifloraceæ*, and a comparison of such cases as those presented by *Passiflora quadrangularis* and *Passiflora fœtida*, placed it, in his opinion, beyond doubt that the function of the extra floral nectaries of the two orders is to attract certain insects—probably ants—which are of service to the plant in protecting it from the attacks of other and harmful insects, such as caterpillars, which are accustomed to creep up the narrow stem for the purpose of devouring or otherwise injuring the young growing shoots. As regards the fertilisation of *Hodgsonia*, the author showed that there were special contrivances to prevent the animal which fed upon the nectar of the flower from obtaining that of the extra floral nectaries, and *vice versâ*, and stated that, considering all the circumstances of the case, it was exceedingly probable that fertilisation was accomplished through the agency of a large night-flying moth.

EDINBURGH

Royal Society, December 20.—Sir W. Thomson, President, in the chair.—The Chairman communicated a paper, by the Rev. J. H. Sharpe, on a remarkable case of stream-lines in two-dimensional fluid motion. The body which produces the stream-lines is symmetrical about an axis, and consists of a semicircular head, with another portion the form of which is given by a transcendental equation.—A note on knots, by Mr. A. B. Kempe, was communicated by Prof. Tait. This paper is preliminary to a detailed investigation of knots by an entirely new process.—Sir W. Thomson discussed the ring-waves produced by throwing a stone into water. This investigation constitutes an extension of Poisson's and Cauchy's results. The wave-velocity is directly proportional to the square root of the wave-length, and the group-velocity is one half of the wave-velocity.—Sir W. Thomson also gave an investigation of the waves produced by a ship advancing uniformly into smooth water. His results show that there is practically no disturbance of the surface outside lines drawn from the ship making an angle of 19° 28′ on either side with the direction of motion. When tested by experiment the angle obtained was 19° 13′.—Dr. T. Muir communicated a paper, by Mr. P. Alexander, on the expansion of functions in terms of linear, cylindric, and spherical, &c., functions by a new and very general method.—In a paper on even distribution of points in space, Prof. Tait replied to certain criticisms made on his results regarding the foundations of the kinetic theory of gases.

Manchester

Literary and Philosophical Society, November 2.—Prof. Osborne Reynolds, LL.D., F.R.S., Vice-President, in the chair.—The following papers were read:—Measurements of the magnetic induction and permeability in soft iron, by H. Holden, B.Sc.—The action of hydrochloric acid gas upon certain metals, by J. B. Cohen, Ph.D., F.C.S.—Capillary constants of benzene and its homologues occurring in coal-tar, by J. B. Cohen, Ph.D., F.C.S., all communicated by Dr. A. Schuster, F.R.S.

Sydney

Linnean Society of New South Wales, October 27.—Prof. W. J. Stephens, F.G.S., President, in the chair.—The following papers were read:—Catalogue of the described Coleoptera of Australia (part vi.), by George Masters. The present part contains all the known Scolytidæ, Brenthidæ, Anthribidæ, Bruchidæ, and Cerambycidæ of Australia, making the total number of species catalogued up to the present time, 6231. The next part, which will be published early in next year, will complete the Coleoptera.—Descriptions of new Lepidoptera, by E. Meyrick, B.A., F.E.S. In this paper descriptions are given of sixteen new species of Australian Lepidoptera belonging to fourteen genera, of which six are new. Among them is *Thalpochares coccophaga*, of which, at the December meeting of the Society, Mr. Masters exhibited specimens of both moths and larvæ, and called attention to the singular habits of the latter, which feed on a species of Coccus infesting a Macrozamia, living concealed in a cocoon-like shelter formed of the exuviæ of the Coccus, and finally pupating therein.—On the flowering seasons of Australian plants, by E. Haviland, F.L.S. This paper enumerates 113 species of plants observed in flower in the neighbourhood of Sydney during the month of July of this year, and is intended to be the first of a series of papers on the subject, by means of which it is hoped that the flowering seasons of at least the plants of the county of Cumberland will eventually be recorded.—Notes on the Rutaceæ of the Australian Alps, by James Stirling, F.G.S., F.L.S. Fourteen species of Rutaceous plants are enumerated as occurring in the region of the Australian Alps, of which one belongs to the genus Zieria, two to Boronia, nine to Eriostemon, and two to Correa. Remarks are also made upon the climatic and other conditions under which the plants occur, and the origin of their specific differences.—On a probably new species of tree-kangaroo from North Queensland, by C. W. De Vis, M.A. The name of *Dendrolagus bennettianus* is proposed for a supposed new species of tree-kangaroo of which one specimen was obtained in the Daintree River District. It lived in captivity for a time, but was subsequently killed, and its skin, unfortunately deprived of everything else but the bones of the hands and feet, was subsequently submitted to Mr. De Vis, who, after comparing it with two skins of *D. lumholtzi*, Collett, has no doubt that it is distinct from its compatriot, and is more nearly allied to *D. dorianus*, Ramsay. As full a description as is possible under the circumstances is given in the paper.—Dr. Ramsay exhibited a specimen of an apparently new species of Monacanthus, presented to the Australian Museum by Mr. G. R. Eastway. He also exhibited eggs of *Ptilonorhynchus violaceus* and *Rhynchœa australis*, and read notes on the subject.—Mr. A. J. North exhibited eggs of *Menura victoriæ*, Gould, from South Gippsland, and of *Geronticus spinicollis*, Jameson, from Hillston, N.S.W.—Mr. Whitelegge exhibited some magnificent specimens of the Alga *Claudea Bennettiana*, Harvey, hitherto known only from one small specimen. It was found abundantly near the Heads of Port Jackson during a recent trawling excursion in connection with the Australian Museum. Some of the specimens taken were nearly 1 foot in diameter. Mr. Whitelegge also exhibited a fine specimen of *Eozoon canadense*, and slides of it and of the above-mentioned Alga under the microscope.

Paris

Academy of Sciences, December 20.—M. Jurien de la Gravière, President, in the chair.—Addendum to the note of December 6 on the conditions determining the form and density of the earth's crust, by M. Faye. In reply to M. de Lapparent's further objections to his theory, the author gives more detailed explanations regarding the phenomenon of compensation between land and water, pointing out that to this compensation is due the persistence of the original ellipsoidal figure of the globe.—On the phosphorescence of alumina, by M. Edmond Becquerel. The experiments here described point to different conclusions from those recently arrived at by M. Lecoq de Boisbaudran, while confirming those deduced from the author's earlier researches.—On some dispositions by means of which bi-refractive photometers may be realised without polarising the light, by M. A. Cornu. Without dispensing with the simpler apparatus of geometrical optics, the author describes several re-adjustments, which enable him to obtain double images of variable intensity in accordance with a well-known law, without having recourse to the employment of polarised light.—Remarks on M. Hugoniot's notes on the flow of gases, published in the *Comptes rendus* of November 15 and 22, by M. G. A. Hirn. The author replies briefly to the objections urged against his conclusions by M. Hugoniot, and still rejects the kinetic theory of gases, which he persists in regarding as one of the most fatal errors of modern science.—Observations in reference to Dr. Philip Paulitschke's "Researches on the Ethnography and Anthropology of the Somali, Gallas, and Hararis," by M. de Quatrefages. These observations, accompanying presentation of a copy of Dr. Paulitschke's work to the Academy, dwell especially on the great scientific interest presented by the mixed Negroid populations of Eastern Africa to the student of anthropology. These peoples are regarded as the outcome of an extremely ancient crossing between the Negro and the White races, the latter being represented chiefly by the African Semites.—Considerations on deep-sea fishes, and especially on those belonging to the sub-order Abdominalidæ, by M. Léon Vaillant. These remarks have special reference to the captures made by the *Talisman* Expedition, which included no less than 3800 true fishes, and which, combined with the researches of Günther, Gill, Cope, Goode, and Bean, already supply materials for a rough classification of these marine Vertebrates.—On the copper present in the grapes and wines yielded by vines treated with various cupreous preparations against mildew, by MM. U. Gayon and Millardet. These researches seem to show that, while the different processes generally exercise some influence on the quantity of copper contained in the grape and vinous fermentation, they appear to have none at all on the quantity of copper which remains in the wine after fermentation. The clear wine, after perfect clarification, contains no appreciable quantity of the metal.—Volume, absolute heat, and specific heat of saturated vapours, by M. Ch. Antoine. Taking a special zero for each vapour, simple formulas are established for working out these several volumes.—Note on the Abelian functions, by M. Appell.—On angular acceleration, a problem of pure kinematics, by M. Ph. Gilbert.—On the flow of elastic fluids, by M. Hugoniot. The author here applies to the flow of saturated aqueous vapour the same method already employed by him in the study of the flow of permanent gases.—Apparatus showing the two modes of reflection of a vibratory movement, by M. J. Violle. The apparatus here described has been constructed by M. König, and is perfectly adapted for demonstrating the method employed by Regnault in his great work on the measurement of the velocity of sound.—On some new properties, and on the analysis, of the pentafluoride gas of phosphorus, by M. H. Moissan. Having already indicated a new process for preparing this substance, the author here gives some fresh results terminating his researches on the phosphorated compounds of fluor.—On the relations of the efflorescence and deliquescence of the salts with the maximum tension of the saturated solutions, by M. H. Lescœur. The conditions of the efflorescence and deliquescence of the salts as determined by Debray are here brought into relation with the maximum tension of the saturated solutions.—Heat of formation of the methylate and ethylate of potassa, by M. de Forcrand.—On the wines and brandies extracted from strawberries and raspberries, by M. Alph. Rommier. By the process here described raspberries are made to yield a wine with over 18 per cent. of alcohol instead of the normal 2 or 2·5 per cent., while the brandy distilled from it retains a highly aromatic flavour. A still more palatable wine, with 16 per cent. of alcohol, is obtained from the fine strawberries grown in the neighbourhood of Paris, the corresponding brandy also preserving the flavour of the fruit.—On the zymotic properties of certain virus: fermentations of nitric substances under the influence of non-aërial virus, by M. S. Arloing. The object of this communication is to show that the virus of non-aërial microbes stimulates the fermentation of albuminoid substances.—Note on the multiplication of *Leucophrys patula*, Ehrenberg, by M. E. Maupas. In a favourable medium, a single individual of these Infusoria, which multiply by fissiparity, is found to increase to over a million in five days. Certain hitherto unobserved irregularities in the process of segmentation are here described.—On the phosphorescence of the Geophili, by M. Macé. As studied on a *Geophilus simplex*, Gervais, this phenomenon appears to be analogous to that of certain Chetopteræ described by Panceri and Jourdan.—On the typical nervous system of the dexter and sinister Prosobranchæ, by M. E. L. Bouvier.—Fresh anatomical and physiological studies on the Glyciphagi, by M. P. Mégnin.—The diseases of the olive, by M. L. Savastano. A brief description of the various forms of hyperplasia and tumours by which this plant is affected.—Remarks on the so-called Calcifugal vegetation, by M. Ant. Magnin. A theory is advanced to explain the presence of these plants in the limestone region of the Jura.—On two rocks in the Vélay and Lyons districts, containing beryl and apatite, by M. Ferdinand Gonnard.—On an experiment undertaken to determine the direction of the currents of the North Atlantic, by Prince Albert of Monaco. The author describes a second excursion on board the *Hirondelle*, during which 510 bottles were thrown into the sea along a course about 500 miles long, and nearly parallel with the twentieth meridian west of Paris. The operation was begun on August 29 and completed on September 5, 1886, and some of the floats have already been picked up at various points on the European seaboard; but the general results are reserved for a future communication.—The periodical showers of shooting-stars and the seismic disturbances of the years 1883, 1884, and 1885, by M. Ch. V. Zenger. During these years, both orders of phenomena are shown to coincide, while they are also frequently accompanied by hurricanes, cyclones, electric discharges, and auroras.

Berlin

Physiological Society, October 29.—Prof. du Bois-Reymond in the chair.—Dr. J. Munk reported on experiments instituted by him in the course of the last two years with a view of arriving at an experimental decision between the two theories of the secretion of urine: the filtration theory of Ludwig, and the secretion theory of Heidenhain. According to the first theory, the blood-pressure prescribed the measure for the urine secretion; according to the second theory, the urine got secreted from the secretory epithelial cells of the kidneys, and the quantity of the matter secreted was dependent on the rate of movement of the circulation of the blood. The speaker had instituted his experiments on excided but living kidneys, through which he conducted defibrinised blood of the same animals, under pressures which he was able to vary at pleasure between 80 mm. and 190 mm. Fifty experiments on dogs whose blood and kidneys were, during the experiment, kept at 40° C., yielded the result that the blood of starving animals induced no secretion of urine, which, on the other hand, showed itself in copious quantities where normal blood was conducted through the kidney. If to the famished blood was added one of the substances contained as ultimate products of digestion in the blood, such, for example, as urea, then did the secretion ensue. The fluid dropping from the ureter contained more urea than did the blood. That fluid was therefore no filtrate, but a secretion. An enhancement of the pressure of the blood flowing through the kidney had no influence on the quantity of the secretion passing away. An increased rate of movement on the part of the blood, on the other hand, increased in equal degree the quantity of urine. On a solution of common salt or of mere serum sanguinis being poured through the kidney, no secretion followed. All these facts, involving the exclusion of the possibility of a central influence being exercised from the heart or from the nervous system on the kidneys, were deemed by the speaker arguments proving that the urine was secreted by the renal epithelial cells. A series of diuretics was next tried, in order to establish whether they operated in the way of stimulus centrally on the heart or peripherally on the renal cells. Digitalis was a central diuretic. Common salt, on the other hand, was a peripheral diuretic. Added in the portion of 2 per cent. to the blood, it increased the quantity of urine eight- to fifteen-fold. Even in much less doses, it was a powerful diuretic. In a similar manner, if yet not so intensely, operated saltpetre and coffeïne, as also urea and pilocarpine. On the introduction, however, of the last substance into the blood, the rate of circulation was accelerated in an equal measure as was the quantity of urine increased, so that in this case the increase in the quantity of urine was, perhaps, exclusively conditioned by the greater speed in the movement of the blood. On the other hand, the quantity of secreted urine was reduced when morphine or strychnine was administered to the blood. In the case of the application of strychnine, the rate in the current of the blood was retarded in a proportion equal to the reduction in the secretion of the urine. The speaker had, finally, demonstrated the synthesis of hippuric acid and sulphate of phenol in the excided kidney as a function of its cells, by adding to the blood pouring through the kidney, in the first place, benzoic acid and glycol; in the second place, phenol and sulphate of soda. In order that these syntheses might make their appearance in the excided kidney, the presence of the blood-corpuscles was not necessary, though, indeed, the presence of oxygen in the blood was indispensable.

BOOKS AND PAMPHLETS RECEIVED

The Origin of Mountain Ranges: T. M. Reade (Taylor and Francis).—The Six Inner Satellites of Saturn; Appendix 1 and 2: A. Hall (Washington).—Conchylien der Gosaumergel von Aigen bei Salzburg: Dr. L. Tausch (Fischer, Wien).—Ueber die Fauna der Oolithe von Cap. S. Vigilio: M. Vacek (Fischer, Wien).—Jahrbuch der k.k. Geologischen Reichsanstalt, 1886 (Hölder, Wien).—Monthly Summaries and Monthly Means for Year 1885, Imperial Meteorological Observatory, Tokio.—Report of the Meteorological Observations for Years 1876-85 at Imperial Meteorological Observatory at Tokio.—The Arithmetic of Electrical Measurements: W. R. P. Hobbs (Murby).—Zoological Record, vol. xii., 1885 (Van Voorst).—My African Home: E. W. Fielden (Low).—Studies in Social Life: G. C. Lorimer (Low).—The Mechanics of Machinery: A. B. W. Kennedy (Macmillan).—A Strain Indicator for Use at Sea: C. E. Stromeyer.—Report on the Progress and Condition of the Government Botanical Gardens at Saharanpur and Mussoorie for Year ending March 31, 1886 (Allahabad).

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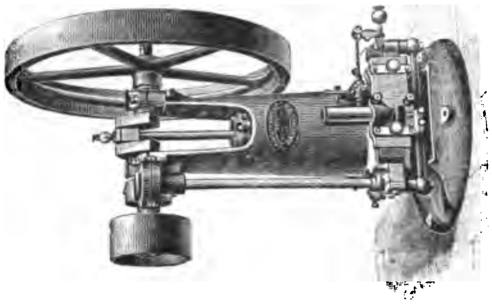

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THE OWENS COLLEGE

The Owens College: its Foundation and Growth, and its Connection with the Victoria University, Manchester. By Joseph Thompson. (Manchester: J. E. Cornish, 1886.)

THE future historian of the progress of education in England during the nineteenth century will regard the foundation of the Owens College, Manchester, as an event of the first importance. The idea of establishing, in the midst of our great manufacturing towns, institutions devoted to the higher learning was not new. The experiment had been tried in various forms in Manchester itself, but had always failed. A College of Arts and Sciences was founded in 1783 by some of the leading men in the town and county, but, owing to "a superstitious fear of a tendency of a taste for knowledge to unfit young men for ordinary business, this excellent institution had not a long existence." In 1836 meetings were held, and a scheme was drawn up for the establishment in Manchester of a College for general education. In the spring of 1837 it was proposed to elect a Medical Faculty in connection with the College; but, before another year passed, the scheme was abandoned, as very few pupils came forward.

Institutions which aimed chiefly at the preparation of candidates for the Nonconformist ministry were more successful. The Manchester Academy, and the Lancashire Independent College, founded in 1786 and 1840 respectively, are active now; but while the latter is in close alliance with the Owens College, the former has migrated to London. It was not the desire of business men for culture or for technical education, but a demand on the part of Nonconformist professional men for a training which the tests imposed by the older Universities debarred them from obtaining there, which enabled these institutions to survive when others failed. Nay, more; the strongest argument which George Faulkner brought to bear on his friend John Owens, to persuade him to alter the will by which he had made him his heir and to found a College, was "that as he had such strong prejudices against the tests imposed at the older English Universities, he could enable young men to obtain an education equal to that of the favoured institutions, without these hindrances."

But, whether or no Owens was influenced mainly by a feeling of indignation at an injustice which has now been long removed, his benefaction has produced results even wider than those which have followed the abolition of the tests he disapproved. The Owens College was the first example of the successful establishment in a manufacturing town of an institution which gave to all comers a University education. When Manchester had proved that success was possible, others were not slow to follow where she had led. The Owens College was opened in 1851. For some years it seemed that it was to share the fate of its predecessors. At length the tide turned, and the passing of the Owens Extension College Act in 1870 marked the attainment of assured success. In that year Newcastle established a College of Physical Science, the Yorkshire College was founded in Leeds in 1874, and now no large town considers its educational equipment complete if it cannot point to a "University College" in its midst. The importance of this result can hardly be exaggerated. In the midst of a great democratic movement, it has been practically proved that culture and learning need not be the exclusive property of the few. The provincial Colleges have made it possible for the young artisan to obtain instruction from, and to test his own abilities by contact with, teachers who are masters of the subjects they profess. The Owens College is, no doubt, as far as its day classes are concerned, a middle-class institution. But its authorities have also developed a system of evening lectures, by means of which many a working-man has made his first step upwards from the ranks.

At first, the Council and Staff of the College had to face the difficulties which beset pioneers. The religious difficulty met them at the outset. An early attempt at amalgamation with the Medical School failed. The Professors one and all complained that, through lack of a sound elementary training, their students were unable to profit by the instruction they gave. "The worst that can be said of [the College]," remarked the *Manchester Examiner* of July 20, 1858, "is that it is too good for us. It is out of place here, just as a missionary may be said to be out of his place on the coast of Africa. He offers the Gospel, and the people want Sheffield blades. . . . The crowd rolls along Deansgate, heedless of the proximity of Plato and Aristotle. . . . And where is poor learning all the while? Going through its diurnal martyrdom of bootless enthusiasm and empty benches."

The men who had the fate of the College in their hands were not, however, daunted by cold comfort such as this. Gaps in the Staff were promptly filled up. Principal Scott resigning on account of ill health, his place was filled by Prof. Greenwood, and Mr. Roscoe was selected to fill the vacant Chair of Chemistry. Nor was this confidence misplaced. Almost contemporaneously with these appointments, the dwindling number of students began to increase, and within some half-dozen years the difficulty of preventing failure was followed by the difficulties of providing for success. In 1864 we hear of the "insufficient or unsuitable accommodation furnished by the College buildings." In 1865 Prof. Clifton reported that the percentage of carbonic acid in the air of his lecture-room at the conclusion of a lecture was more than four times the maximum consistent with health. Students of the biological sciences complained of the difficulty of finding admission to well-arranged and complete collections of natural history.

It would take too long to follow step by step the advance of the College, but in reading Mr. Thompson's full account of the way in which each difficulty was overcome, the reader cannot but be struck with the fact that the work of the Council and Staff was essentially that of pioneers. Everything had to be discussed; nothing was determined by precedent. In 1860 it was decided that a Professorship of Physics should be founded, but before this step was taken it was thought necessary to inquire not only whether the students were numerous enough and the College rich enough to warrant the change, but whether a Professorship of Physics was in itself a desirable thing. Professors De Morgan and Stokes were asked for formal written opinions on this knotty point. The new Chair was only established when it was held to have been proved that the field of mathematics and natural philosophy was wide enough for the employment of two labourers in different parts. There can be no doubt that the College was remarkably fortunate in the members both of its Council and of its Senate. The Neilds—father and son Mr. Ashton, Mr. Oliver Heywood, and many others, worked as though their personal interests were all bound up in the success of John Owens' bequest. They succeeded in filling the Chairs with men of the most brilliant abilities. To mention only some of those whose connection with the College has now ceased, it is evident that an institution which has within a few years commanded the services of men like Sir Henry Roscoe and Professors Frankland, Clifton, Jevons, Jack, and Gamgee, must have deserved the success it has won.

Nor is it uninstructive, regarding the Owens College as the pioneer in a great movement, to observe how largely that movement has been and is being directed by men who themselves owe much to the Manchester institution. By glancing only at the list of those who have held the office of demonstrator in the physical or chemical laboratories, or of lecturer on mathematics, we observe that the Professorships of Physics in the Mason College, Birmingham; of Chemistry in Anderson's College, Glasgow, the Yorkshire College, Leeds, the College of Physical Science at Newcastle-upon-Tyne, and the Firth College, Sheffield; and the Professorships both of Chemistry and Physics in the Normal School of Science, South Kensington, and in University College, Dundee, are or have been held by *alumni* of the Owens College.

When once the College began to grow, it grew rapidly. The change wrought in fifteen years was remarkable. Besides the establishment of numerous new professorships, the Manchester Natural History and Geological Societies handed over their collections to the College as the recognised centre of scientific learning in Manchester. The College and the Medical School were fused into one institution. The handsome buildings in Oxford Road were erected upon land, and partly also with funds, furnished by an Extension Committee, which numbered among its members many of the best-known men in the town, and of which Dr. Watts, whose death Manchester has recently had to deplore, was secretary.

When this was accomplished, the College proceeded to claim the status and powers, as it had already proved its capacity for doing the work, of a University. This question affected interests other than those of Manchester itself. The Yorkshire College, Leeds, then very recently founded, and immature, had sufficient faith in its own future to claim a share in determining the conditions on which a new University was to be established in the North of England. This claim was at first enforced by a formal opposition to the Owens College scheme, on certain specified grounds. Whether a Leeds historian would agree with Mr. Thompson that the opposition "was not based upon accurate knowledge" may be open to question. It may be that the mistake was on the other side, and that the Owens College authorities had not as yet fully recognised the magnitude of the movement to which they themselves had given so great an impulse. However this may be, the controversy seems to have been conducted with fairness and good temper, and to have resulted in an agreement which left none but kindly feelings behind. The Council of the Owens College received satisfactory assurances that their Leeds friends did not desire to share the advantages to be conferred by the Charter of the University till they had made their College worthy of representing it in Yorkshire. On the other hand, they conceded the demand of Yorkshire that the name of the University should not be that "of a town or of any person whose claims to such distinction are merely local." A compromise was arrived at on the other point to which importance was attached. Leeds desired that the Governing Body of the University should be separate and distinct from that of the Owens College, and that it should have power "to incorporate the Owens College and such other institutions as may now or hereafter be able to fulfil the conditions of incorporation laid down in the Charter." This condition was agreed to, with the modification that the Owens College should be named in the Charter as the first College of the University, and that thus its incorporation should be simultaneous with and not posterior to the foundation of the University itself. Finally, a joint deputation from Lancashire and Yorkshire petitioned the Crown in favour of the establishment of the new University. The Charter was granted in 1880. The Owens College was constituted the first College of the University, and though the Yorkshire College has not yet joined it, University College, Liverpool, has recently been admitted as the second member of the federation.

Mr. Thompson's work appropriately ends with the flattering tribute paid by his fellow-citizens to Sir Henry Roscoe by his election as Member of Parliament for one of the divisions of Manchester, and with the expression of the regret of the Council at the severance of his connection with the College. The success of the Owens College is due to no one man, but to no one man more than to Sir Henry Roscoe. His withdrawal may fitly mark the termination of the period of struggle with initial difficulties. The main work of organisation is over. The chief outlines of the scheme for bringing industry into contact with culture and with technical education are drawn. It remains for the Owens and her sister Colleges to fulfil the task on which they are now fairly embarked.

We believe it is not unlikely that in carrying on their work the authorities of the Victoria University may find it necessary to appeal to the Government for a contribution towards the University funds. It will of course be easy to say that as Manchester and Liverpool have done so much they may be left unaided to do more. In forming an opinion upon this question it will be well for the public to remember that, whereas the Universities or Colleges in Edinburgh, Glasgow, Aberdeen, St. Andrews, Aberystwith, Bangor, Cardiff, and the Queen's Colleges in Ireland, receive subventions from the State which amount in all to between 40,000*l.* and 50,000*l.* per annum, English higher education in provincial towns has been entirely provided by unselfish private enterprise, with some assistance in Newcastle from the University of Durham, and elsewhere from the Clothworkers' Company and others of the City Guilds. As far as help from Parliament is concerned, England as usual has to find its share of the money and reaps none of the advantage. It is surely not too much to hope that, if 8000*l.* a year is allotted from the public funds to Colleges which supply the wants of Aberystwith and Bangor, it will not be considered impolitic to help from the National Exchequer the magnificent and national work which has been and is being done in the North of England.

But to return to the book before us. It is fortunate that while the memory of the men who initiated the undertaking is still fresh, and while most of those who carried it out are still with us, an historian such as Mr. Thompson has been found for the Owens College. He has given, not merely the public story of the institution, but short histories of many of those who were closely connected with it and who have now passed away. He has evidently had access to authentic documents and other sources of accurate information, and he has produced a work which will be read with interest by many who have in the past known but little of the Owens College and its founders.

ANDERSON ON HEAT AND WORK

On the Conversion of Heat into Work. By W. Anderson, M.Inst.C.E. (London: Whittaker and Co., and George Bell and Sons, 1887.)

VERY few modern books on Engineering contain what can truly be called accurate science; one of whose special characteristics is the use of each term *in one definite sense only.* In other words, take at random a work on any branch of Engineering, and you find, in every page, more than one sentence which, if it be read as a scientific statement, is simply inaccurate. Of the exceptional works the majority consist of those written by the late Prof. Rankine. He seems to have left no successor, so far as this department of applied science is concerned; and the book before us strongly supports the notion. It gives, in a succinct but comprehensive form, an introduction to the modern Dynamical Theory of Heat, treated almost entirely from the practical Engineer's point of view. It is obviously written by a man who knows his subject, and it is therefore presumably written in terms intelligible to those for whom it is designed. It thus affords a good opportunity of making some further remarks upon the strange line of separation which has unfortunately been drawn between the vocabularies (or, rather, the dictionaries) of Pure and of Applied Science. In using this opportunity, for the purpose stated, we do not attack the present work in particular; we attack the mass of works on Engineering, of which it is a high-class specimen.

From the purely scientific point of view there are two prominent faults in the majority of such works as that before us. The first is the habitual use of a special "vernacular"; not so outrageous, perhaps, as "pidgin" English, but quite on a par with a "wire," a "cable," an "aniline," and such-like monstrosities of recent American origin. Were the words of this vernacular different from those of strict science, our only complaint against their use would be that we should have to learn what would be practically a new language before we could read an Engineering book. But they are, in the main, the same words; and yet each stands for other than the usually accepted meaning. Thus we constantly find pressure given as so many tons per square inch (sometimes the word "square" is omitted). Now, tons per square inch, or pounds per square foot, refer to matter and not to force. They measure, in fact, what is called surface-density. This is altogether "most tolerable, and not to be endured."

The second fault is more grave. It consists in the fundamental misuse of well-settled scientific terms, which the author of an Engineering book usually perpetrates whenever (for a moment) he deserts his vernacular and passes from the applied to the pure part of his subject. We remark in passing that, in the very first page, our author speaks of Mayer's EXPERIMENTAL demonstration of the equivalence of heat and work! This shows how deep a root has been taken by the extravagant laudations of Mayer, which were so common twenty years ago, but which have long since been thoroughly exploded.

Now, to faults of the first class mentioned above. We quote only a few of the more racy passages we had marked; and here, as in the subsequent examples, we introduce (to save comment) Italics where they seem desirable.

"Accelerating forces, *that is* forces acting steadily for a time." This may be the Engineer's vernacular, but it has no necessary connection with the use, in English, of the term "accelerating force."

"Dividing (3,942,400 *foot-pounds* per minute) by 33,000 *foot-pounds*, we get 119·4 *horse-power!*" Put this in the form "dividing 500*l.* a year by 50*l.* we get 10*l.* a year," which contains essentially the same absurdity, and we can scarcely fancy that our author would have let it stand; although in its above form it is put as Engineers too commonly put it.

"The heavenly bodies, *moving at uniform velocities for ever* are instances of *potential* energy." This is the Engineer's way of saying that since there is no change of kinetic, there can be no change of potential, energy.

"When we speak of perfectly elastic substances, we do not mean those which, like india-rubber, have a great range of elasticity." But, though this is true of Engineers, it is not true of purely scientific men. Think of air, for instance.

"Endowed with energy competent to produce the *sensation* of 100° Fahr. of *heat.*" Here the Engineers pick a quarrel with the Physiologist as well as with the Physicist.

As explained above, all this is merely the licence which practical men take with scientific terms. As the book is written for such men, perhaps we ought not to complain. But the faults of the second class, some of which we proceed to give, can only be explained by the practical men's using scientific words in a wrong sense. From the following, and others too numerous to be quoted, a new science (!) could be founded, having nothing in common with that which Galilei and Newton have handed down to us, (of course on the supposition that all the words employed are to be taken as they are understood in pure science.)

"Hence the *potential energy* of each gallon of water is

1/330 of a *horse-power!*" Our author would surely have thought twice before writing "the *distance* from London to York is 200 *miles per hour*." But this, to which we invite his careful attention, would have involved no greater blunder.

"If the bodies are elastic . . . , the *whole* of the *energy* of the striking body is expended in producing *motion* in the body *struck*." After this it will not, perhaps, surprise the reader to find that we are furnished with a calculation of "the total *heat from absolute zero* resident in exploded powder, at an atmospheric temperature of 50°, or 510° absolute."

The work may be made, even as a whole, thoroughly useful to those habituated to the persistent inaccuracies of the "vernacular":—but, to effect this, it must be carefully purged of statements analogous to the three last-made quotations. P. G. T.

A FIELD NATURALIST IN EASTERN BENGAL

Letters on Sport in Eastern Bengal. By Frank B. Simson, Bengal Civil Service, retired. (London: R. H. Porter, 1886.)

THIS book is essentially a record of sporting scenes, and the author has ostensibly written it for the purpose of giving instruction in the art of shooting and hunting wild birds and beasts of various kinds, from quail and snipe to tigers and rhinoceroses. Yet it contains so many good observations on the haunts and habits of wild animals, and the author shows himself so capable a field naturalist, that a reader who looks for zoological information will probably be disappointed at not finding more novelty. The scene of Mr. Simson's principal adventures, Eastern Bengal, a vast plain traversed by mighty rivers, a country of rice fields and cane brakes, and great grass jungles, of "bheels," or marshes, and "churs," or temporary islands and sand-banks in rivers, has by no means been rendered too familiar by description. Fertile and peaceable, with never-failing rains and magnificent water communication, it furnishes few sensational paragraphs for newspaper correspondents or other manufacturers of periodical literature. The region is as little known to Anglo-Indians in general as the Highlands of Central India, so vividly described by Forsyth, or the wild Mysore country, of which the elephants, tigers, and other wild beasts found an historian in Sanderson. Why is it that the additions to our zoological knowledge made by Mr. Simson are so much less important than those made by Sanderson and Forsyth?

The explanation is probably twofold, if not threefold. All the three writers named were enthusiastic sportsmen and good observers, but Forsyth and Sanderson related events of more recent date, the details of which were naturally more vivid, whilst the present work is a series of reminiscences, written out long after the incidents described took place. The avocations of the different writers, too, were very dissimilar. Those of the two authors first named led them to pass weeks and months amongst the haunts of wild animals, whilst Mr. Simson, a Bengal civilian, could only spend an occasional holiday at a distance from his office, or avail himself of a few hours at a time during the cold season's tour. Another reason, perhaps of even more importance, is the great difference in the nature of the country, and the different system of hunting rendered necessary. The great grasses of the Gangetic plain, even when reduced to patches by the fires of the spring, conceal the movements of their inhabitants, from rhinoceroses and buffaloes downwards, far more than do the jungles of Central and Southern India, especially after their much less luxuriant grasses have been burnt. The process of beating out a patch of thick grass 10 to 20 feet high with a line of elephants differs widely in the opportunities afforded for observation, from the tracking, chiefly on foot, of the animal sought after, through the burnt glades of the Satpura hills or the comparatively thin undergrowth of the Sahyadri forests.

It must not be supposed that Mr. Simson's work contains no novel observations. A very large proportion of our acquaintance with the habits of animals, especially of the larger Mammalia, is due to sportsmen, but the value of their observations varies. A few sportsmen are deliberately untruthful,—these are easily detected; but many more are unqualified for accurate observation. There is no better test of a writer's truthfulness and capacity than his snake stories. In America it is commonly said that there is no subject on which ordinary mortals are so prone to what may euphemistically be termed "romancing." Indian experience corresponds to American, and whatever may be the deficiencies of the Bengali peasant, no one ever credited him with want of imaginative power. The Europeans in India, as a rule, know nothing about snakes. In the work under review, two of the best letters (the whole is written in epistolary form) deal with snakes, and the account of the poisonous species, all of which are correctly named and described, is excellent. To have picked out the grains of truth, and disregarded the chaff, shows judicial acumen worthy of one who has, in the administration of the law, had much experience of conflicting evidence. One of the most interesting facts mentioned is the use made of cobra venom in poisoning arrows used for the destruction of wild animals.

Another interesting observation may be noted: the shooting of a tiger "whose paunch was crammed full of grasshoppers or locusts." Hitherto, although tigers were not thought to be very particular, they were supposed to draw a line at frogs, and were not suspected of condescending to devour insects.

The plates are good on the whole, the elephants excellent, and the lithograph entitled "A scrimmage with a tiger," is one of the best representations of the animals and men depicted to be found in any Indian sporting work. But the pigs—with one exception, in which the animal looks as if he had been shaved—are far too shaggy, and resemble the European boar, *Sus scrofa* rather than the Indian *S. cristatus*. W. T. B.

THE MEASURE OF THE METRE

La Mesure du Mètre. By W. de Fonvielle. (Paris: Hachette, 1886.)

M. DE FONVIELLE'S little volume is truly national. From one end to the other it rings with applause for those brave men of France who, in 1792 and the following years, under extraordinary difficulties—political, social, and geographical—determined the measure of the arc of the meridian from Dunkirk to Barcelona; from the 1/10,000,000th part of which arc the measure of the metre was then derived.

The author in stirring language recounts the dangers and disappointments of the scientific men engaged in this work during the Revolution—Méchain, Delambre, Berthemie, Biot, Arago, Lenoir, and Lavoisier. He endeavours to awaken a warm and genuine admiration for their labours, and to show that the love of science is in some way natural to France. In pathetic words he recounts the dangers in the field: Méchain's work in Spain, his troubles at home, his recall, and return to Spain; his fears that the great measurement might never be accomplished; and at last the sad end—Méchain's death in 1807 at Plana, a victim to yellow fever. Then follows the appointment of Biot and Arago, and the account of their doubts and difficulties in completing the measurements, of the capture and imprisonment of Arago and Berthemie, of their detention in Algiers, and of their ultimate ransom and release.

M. de Fonvielle traces the development of the new system of weights and measures from the proposal of Buffon in 1790 to take as a unit the length of the seconds pendulum, to the report of the completion of the measurement of the metre in 1809. He refers to the invitation given by France in 1790 to our country, to join in an international effort to adopt one weight and one measure for all nations. This invitation, as experience has shown, ought not to have been declined, but even now, owing to the reluctance of English-speaking nations to abandon their traditional units, a similar proposal might possibly not be warmly received.

M. de Fonvielle reminds his readers that the French metric system must not be altogether regarded as a French innovation, for the Chinese long ago adopted a decimal system. It is to Shun, the sage, when Regent of the Chinese Empire, B.C. 2287, that China owes its decimal system, based on a so-called natural constant, the length of the musical standard lü, or bamboo pitch-pipe.

Of course this little volume is intended for popular reading, particularly in France. For the true account of the circumstances and results of the measurement of the arc of the meridian which passes through Paris, we must go to the "Mémoires" published by Méchain and Delambre in 1806, and to the observations of Biot and Arago issued in 1821.

OUR BOOK SHELF

Histoire Générale des Races Humaines. Introduction à l'Étude des Races Humaines—Questions Générales. Par A. de Quatrefages, Membre de l'Institut. (Paris: A. Hennuyer, 1887.)

PROF. DE QUATREFAGES and M. E. T. Hamy propose to edit a general history of the human race, and the present volume, by Prof. de Quatrefages, is intended for an introduction to a series of monographs by various authors. The dark races will be described by M. E. T. Hamy; the yellow races by M. J. Montana; and the red races by M. Lucien Biart. These volumes are in course of publication, and the first volume of the series, on the Aztecs, by M. L. Bert, has already appeared. There will be a volume on the Mongols, by M. J. Deniker, and one on the Foulahs, by Dr. Tautain.

In the present volume the general questions of ethnology are treated of, and the subject of the classification of the human race is passed in review. With that charming style which characterises the writings of this author, and which has for long made him one of the most popular writers on scientific subjects in France, he here gives a *précis* of the chief works treating on ethnology, and decides that the human race must not be placed in the same category with the animal race, because it exhibits the presence of two additional phenomena, those of morality and religion.

On the question of the unity of the human species, too often one of mere words, the *pros* and *cons* are placed before the reader in a tabular form. In the chapter on the first appearance of man, the various transformistic theories are passed in review, and the views of Darwin, Huxley, Vogt, and Haeckel are alluded to; but the author for himself believes that any certain knowledge on this point is beyond our actual powers. In other chapters, the antiquity and geographical origin of the race are treated of, as well as the subject of the peopling of the globe and the acclimatisation of the species. Primitive man is regarded as of distinct ethnic types, and from these the races took their rise. Lastly, the physical, the intellectual, and the moral and religious characters of the races are discussed in some detail. The work is supplied with numerous and excellent illustrations; it is printed in clear type on royal octavo paper, and forms a handsome volume of nearly 300 pages.

Grundzüge einer Theorie der kosmischen Atmosphären mit Berücksichtigung der irdischen Atmosphäre. Von Wilhelm Schlemüller. (Prague.)

IN this pamphlet the author introduces a modification into the ordinarily accepted dynamical theory of gases by assuming that the molecules of a gas at uniform temperature are all affected with absolutely the same linear velocity as regards magnitude, instead of the temperature being dependent on the mean or average velocity. This of course greatly simplifies the labour of deducing the fundamental relations between pressure, density, temperature, and the potential of external forces; and he claims to be able to deduce the relation, which for the terrestrial atmosphere gives Bessel's refractions to 90° (*sic*) zenith distance; agreeing with the formulæ found by Bauernfeind in 1862-64. We may remark that the convertible equations are reproduced in some cases with almost wearisome frequency, and that Joule is twice called Jonle.

Manual of Physical Geography of Australia. By H. Beresford de la Poer Wall, M.A. (Melbourne: Robertson.)

THIS little manual is written for Australian schools, and may be accepted as a fair and trustworthy account of the physical geography of Australia. For an exhaustive treatment of the subject the material is still wanting for a large section of the continent; on others, again, there is abundance of material, and of these Mr. Wall has made creditable use. It is a pity the book should be burdened with such terrible lists of names as those on pp. 9 and 10: the author would have done much better had he shown the relations of the leading capes to the general relief of the land.

An Intermediate Physical and Descriptive Geography, abridged from the Physical, Historical, and Descriptive Geography of the late Keith Johnston. (London: Stanford, 1886.)

THE late Keith Johnston's larger geography is on the whole the best general text-book of the subject in English. The present abridgment for middle-classes in schools seems to us judiciously done.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Mr. Wallace on Physiological Selection

SEEING that Mr. Wallace has now changed front with regard to some of the points at issue between us, I must once again address you upon this subject.

(1) He appears to have forgotten that the whole plan of his original impeachment consisted in representing me as an arrogant heretic. This impeachment was published under the heading "Romanes *versus* Darwin," and point by point it laboured to show that I was deserving of excommunication as a rebel against the highest authority. In my reply, therefore, I was obliged to show that the charge was misdirected; and this I did by simply quoting passages from that highest authority himself. It is needless to say that I am now as much satisfied as surprised to find this charge, not only abandoned, but reversed. Whereas I was previously accused of presumption for disregarding authority, now the remonstrance is—"he appeals to authority against me," and "I decline to accept authority as an infallible guide." So do I. But I quoted my authority merely for the avowed purpose of defending myself from the specific charge of my opponent. It was *he* who appealed to Cæsar, and cannot therefore now complain if to Cæsar he had to go. Truly, if I may employ his own mode of expression, "further discussion of the matter with such an adversary is out of the question."

(2) But, as regards one of the points, he says that my quotations appear to him to support his own views rather than mine. The shortest way of testing the value of this judgment will be to print in succession three passages, which I have selected as serving in each case most concisely and most fairly to embody the opinion of its writer. The point in question is as to whether specific characters are "invariably" adaptive, or "frequently" not so, and the italics are mine.

"When, *from the nature of the organism and of the conditions*, modifications have been induced which are *unimportant* for the welfare of the species, they *may be*, and *apparently often have been*, transmitted in nearly the same state to numerous, otherwise modified, descendants." (Darwin, "Origin of Species," p. 175.)[1]

"I believe, therefore, that the *alleged* inutility of [many] specific characters claimed by Mr. Romanes as one of the foundations of his new theory, *has no other foundation* than our extreme ignorance." (Wallace, *Fortnightly Review.*)

"The matured judgment of Mr. Darwin clearly recognised the distinction between the origin of *species* and the origin of *adaptations*,—a distinction, indeed, which necessarily follows from his repudiation of the doctrine of utility as universal. . . . . Therefore, with him I believe that an incalculable number of specific characters are of an adaptive kind, and that many more which now appear to us (in our ignorance) to be useless, will hereafter be proved to be useful. But with him also I believe that *a large proportional number* of such characters actually are destitute of utility, having been due, as he says, to 'fluctuating variations, which sooner or later became constant through the nature of the organism and of surrounding conditions, as well as through the intercrossing of distinct individuals; *but not through natural selection.*'" (Myself, *Nineteenth Century.*)

(3) "The impossibility of proving a negative is proverbial, but my opponent declares that his negative—the uselessness of specific characters—wants no proving, but must be accepted till in every case the affirmative is proved." Now, I have made no such declaration. My statement was: "It is too large a demand to make upon our faith in natural selection to appeal to the argument from ignorance, when the facts require that this appeal should be made over so large a proportional number of instances." It is really Mr. Wallace who declares that his *affirmative*—the *invariable* usefulness of specific characters—wants no proving, but must be accepted till in every case the negative is proved, notwithstanding that, as he allows, "the impossibility of proving a negative is proverbial." Of course, *if it has been previously assumed* that natural selection is the *only* factor of organic evolution, we are entitled to conclude that the doctrine of utility as universal requires no further proof, since it follows deductively from the assumption. But where the very question in dispute is as to the validity of this assumption, it becomes an almost comical instance of circular reasoning to construct our biological catechism thus:—Why do you believe that natural selection is the only factor of organic evolution? Because I know that in organic Nature utility is universal. But how do you know this, seeing that "our extreme ignorance" renders it impossible to suggest, in a vast number of cases, what the utility can be? Because I have already proved that natural selection has been the only factor at work.

(4) Mr. Wallace imports from the monthly periodicals part of our discussion on the swamping effects of intercrossing. Here therefore, I must follow him. In my Linnean Society paper I had urged that natural selection must be seriously handicapped in its action by the swamping effects of fortuitous variations intercrossing with their parent forms. This statement Mr. Wallace contradicted on the ground that Mr. J. A. Allen had furnished "a complete demonstration of individual and simultaneous variability by a series of minute comparisons and measurements," with the result of showing that, whatever modification might be required, "we *always* (italics his) find a considerable number, say from 10 to 20 per cent. of the whole, varying simultaneously, and to a considerable amount, on either side of the mean value." Now, in my reply I pointed out that all the variations thus recorded by Mr. Allen were of a kind which had "nothing to do with the difficulty," seeing that they had reference only to such features as "size, strength, fleetness, colour, relative proportions of different parts, and so on, all of which—as we well know without going beyond the limits of our own species—are so highly variable as never all to be precisely the same in any two individuals." Then, by way of illustration, I said: suppose "it were required to produce a breed of race-horses with horns upon the frontal bone, . . . or a fighting spur on a duck, clearly it could not be done by natural selection alone" in the latter case, or by artificial selection in the former; the principle of selection would here require to be assisted by "some common cause [of variation] acting on a number of individuals simultaneously." But there was nothing in the use of this illustration to provoke the remark that it indicates "the belief, apparently, that these are a class of characters which are distinctive of closely allied species"—although such does happen to be the case as regards certain allied genera. I merely requested Mr. Wallace to show me his "considerable number of specimens diverging from the mean condition," as regards either of these structures, *however incipient*—or as regards any other structures, save those the general variability of which as to relative size, &c., no one would dream of disputing. And this I still hold he is obviously bound to do, if he is to sustain his sweeping statement that whatever modification of type may be required, we *always* find from 10 to 20 per cent. varying in the needful way. Thus, as a mere matter of dialectic, I confess it appears to me a somewhat unaccountable expedient to affirm that my *reductio ad absurdum* is "preposterous"—such happening to be the very quality which this mode of refutation is ordinarily designed to present.

(5) Lastly, my critic says:—"The argument to show that the supposed physiological variations would be perpetuated, seems to me as weak and unsatisfactory as ever." This may well be. Indeed, I never supposed that anything would be likely to influence the judgment of Mr. Wallace where natural selection is concerned. But I did not write with any such object. I wrote merely to dispose of a particular criticism which he had advanced, and there can be no two opinions as to the result. For I have shown that whatever may be thought about the truth or falsehood of my theory,[1] at least it is certain that it *cannot* be affected by the criticism of Mr. Wallace; and this for the simple reason that he has run a tilt, not against my theory at all, but against a completely different theory, which, like a figure of straw, he had himself set up. Now that he can no longer have any doubt as to what my theory is, I willingly conclude that he must still have some reasons for thinking it improbable that the supposed physiological variations (if they occur) should be perpetuated. But I am free to confess that it passes my powers of conception to divine what these

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[1] By a curious and undesigned coincidence, the same issue of NATURE which contains Mr. Wallace's letter also contains my review of Mr. Spencer's essay on the "Factors of Organic Evolution." In that review several other passages are quoted from Mr. Darwin's works to the same effect.

[1] I call it my theory, because I now understand that it differs widely from that of Mr. Catchpool (see NATURE, vol. xxxiv. p. 617).

reasons can be: I only know that they must be of a totally different order from those which constituted the substance of his published criticism.

Of course the question whether or not these physiological varieties *do* occur is quite distinct; and I most heartily agree with Mr. Wallace that this is a question of fact which ought to be decided, before it can be worth anybody's while to attack my suggestion upon any other grounds. If Mr. Wallace had seen this in the first instance, he might have saved both himself and me a good deal of trouble; but at the same time he would have deprived me of no small amount of encouragement. For I am now more than ever satisfied that the suggestion does not admit of being assailed on any grounds of general reasoning; but, on the contrary, that as a theory it is antecedently probable, and can only be refuted—if it is to be refuted—by an appeal to fact in the form of experiment. And as I cordially hope that this may be the last time that I shall have to address you upon this subject, I should like to neutralise the discouraging influence on experimental verification which may have been exercised by premature criticism in your pages. This I hope in some measure to effect by making two remarks. The first is that my own estimate of the antecedent probability of the theory is shared by some of the highest "authorities" on the Continent. The second is that, in all the lines of inquiry hitherto pursued, I find striking evidence of the actual occurrence of the physiological varieties in question. But as this evidence requires to be largely supplemented by experiment, and as every experiment requires at least three years to perform, those biologists who think with Mr. Wallace may be glad to hear that it will be a very long time before I shall have occasion again to trouble them with the theory of physiological selection.

GEORGE J. ROMANES

The Alleged Ancient Red Colour of Sirius

WITH reference to your paragraph last week (p. 378), in the "Astronomical Column," on "The Alleged Ancient Red Colour of Sirius," it does not seem to have been noticed that the early observations of Sirius were made at its heliacal rising. Under these circumstances the sun is a red star.

F.R.S.

A Green Light at Sunset

AT sunset to-night I observed a phenomenon which has, I believe, been seen from on board ship, but never probably from a place with such a distant sea-horizon as we have here—some seventy miles. The sky for a short distance above the point where the sun set was perfectly clear of cloud or haze, and I watched carefully the last portion of its disk disappear into the sea. As soon as the last speck of the yellow vanished, a momentary bright green flash shone out. This was quite different from the complementary green seen after looking at the setting sun; brighter and bluer in tint. I have seen it stated that the cause of this green light is the sun shining *through* the water that hides it, and would be glad to know if such is the true explanation.

R. T. OMOND

Ben Nevis Observatory, February 12

Sunset Phenomenon

ON February 21, at 5.25 local time, my attention was attracted by a bright red glow reflected from the earth outside a window having an eastern aspect.

On going out of doors to the Observatory, it was evident that this crimson light proceeded from a band of cloud about 10° in width forming a great circle in the heavens, and intersecting the horizon at points, as well as I could estimate, 145° W. and 35° E. of true north, the inclination of this great circle to the horizon being about 15° or 20°.

In less than three minutes, before I could reach the Observatory, the magnificent spectacle had completely vanished, and in the place that it had occupied were merely some streaks of cirrus and cirro-stratus, the latter being nearest to the place where the sun had set, and in half an hour the entire heavens were cloudless.

Will reflection, or refraction, or both, suffice to explain the above?

WENTWORTH ERCK

Shankhill, Co. Dublin

Aspects of Clouds

IN Mr. Ruskin's "Modern Painters" (i. Part 2) I have noticed the following passage amongst the author's remarks on the aspects of clouds:—

"I have often seen the white, thin, morning cloud edged with the seven colours of the prism. I am not aware of the cause of this phenomenon; for it takes place not when we stand with our backs to the sun, but in clouds near the sun itself; irregularly and over indefinite spaces, sometimes taking place in the body of the cloud. The colours are distinct and vivid, but have a kind of metallic lustre upon them."

And again, the author describes the "scattered mists rallying in the ravines and floating up towards you along the winding valleys till they couch in quiet masses, *iridescent* with the morning light, upon the broad breasts of the higher hills."

Dr. Johnstone Stoney recently read a paper to the Royal Dublin Society entitled "The Iridescent Phenomena in Clouds," wherein he explains the cause of a somewhat similar appearance which clouds at times present. Their outer portions are suffused with soft shades of colour like those of mother-of-pearl, a lovely green being generally conspicuous. The tints are usually distributed in irregular patches as in mother-of-pearl, but in some cases they form a regular fringe. Dr. Stoney explained that these phenomena are due to particles of ice, in the form of crystals of various sizes and shapes, and according to their position and character the sun's rays are reflected through them in various colours, thus producing the beautiful effect.

Would this be an explanation of the appearances to which Mr. Ruskin refers?

ROBERT JAMES REILLY

Boyle, Ireland, February 17

A Recently-Discovered Deposit of Celestine

IT may, perhaps, be worth mentioning that a large and valuable layer of celestine has been lately found at Yate, in Gloucestershire. It lies just below the sub-soil upon a bed of red Triassic marl, which rests unconformably upon the coal-measures, just at the eastern edge of the Bristol coal-field in that district.

The deposit is, for the most part, about half a metre or more in thickness, and consists chiefly of loose nodules which, when broken, are seen to be masses of white, crystalline, nearly pure celestine. Geodes are occasionally found, one of which, about 15 cm. in diameter, lined with fine clear crystals, is now in our school museum. Beautifully transparent, though not well crystallised masses of selenite also occur in the deposit, and in these are sometimes inclosed single detached crystals of celestine. I picked out one crystal (though it seemed almost sacrilege to break up the fine specimen of selenite), which is about 7 × 4·5 × 1·5 cm., doubly terminated, fairly clear, and showing very perfect, well-developed faces. Its density is 3·95, and it shows very perfectly the characteristic light-bluish tinge of celestine.

It would, I think, be quite worth while for any mineralogist who happens to pass near Bristol to pay a visit to the place, which is only about twelve miles distant on the Midland Railway to Gloucester. The extent of the deposit is not known, but when I was there in October last, and again at Christmas, it was being worked in several fields north-east of the church, about a mile and a half from the station. Large quantities are being sent away, of course for the purposes of sugar-refining and adulterating white-lead paint.

H. G. MADAN

Eton College, February 21

"Culminating Sauropsida"

IT is with satisfaction that I note in NATURE of February 3 (p. 331), that Prof. W. K. Parker finds it more and more impossible "to conceive of birds as arising *direct* from the Dinosaurians, or indeed from any other order or group of reptiles." The sentence, no doubt, suggests an indirect origin of birds from reptiles; but, further on, Prof. Parker explains that if protovertebrate forms existed it is quite supposable that a metamorphosis may "have taken place of this and that *quasi*-larval form into archaic reptile, ancestral bird, or primitive mammal." We must therefore conclude, either that there were two kinds of protovertebrates, namely, piscine and reptilian—or ichthyopsidan and sauropsidan, as Prof. Parker would probably prefer to call them—fundamentally distinct or preceded by common ancestors, and in neither case themselves entitled to be called protovertebrates, or else that the protovertebrates referred to were ichthyopsida, that is to say, more simply, allied to the amphibia. I do not object to that latter supposition. I suggested it myself in 1884 (*Journal of Anatomy and Physiology*, xviii. p. 356), as perhaps Prof. Parker is aware. But if birds are developed from amphibians or pre-amphibians, and if Prof. Huxley is right, as I believe he is, in supposing that the connection of mammals with amphibians is neither through reptiles nor birds, we come to this: that amphibians or pre-amphibians have furnished the common stem whence reptiles, birds, and mammals have diverged. In that case there is an end of that group, "Sauropsida," which the birds are alleged by Prof. Parker to "culminate."

But, further, amphibians are certainly more closely allied to reptiles than to either birds or mammals. Cuvier's system may therefore be justly reverted to, and the Amphibia or Batrachia be considered as the lowest division of the Reptilia, which I do not for one moment doubt is the true classification.

University, Glasgow, February 8 JOHN CLELAND

The West Indian Seal (*Monachus tropicalis*)

IT will probably be of interest to the zoological portion of your readers to learn of the re-discovery—or the full discovery—of the West Indian seal (*Monachus tropicalis*). The history of this pinniped is in brief as follows.

It was first noticed by Columbus in his account of his second voyage (1494) as having been found in some numbers on the rocky isle of Alta Vela, off the southern shore of Hispaniola, where his sailors killed eight of them for food. Later—in 1675—Dampier found this seal in abundance on the Alacram reefs, about 80 miles north of Yucatan. At that time it was killed there in great numbers for its oil.

The seal then remained unnoticed for over a century and a half, having no place whatever in the writings of zoologists until 1843. Then Mr. Richard Hill published an account of it in the "Jamaica Almanac," calling it the Pedro seal, from the Pedro Keys, some 60 miles south of Kingston, Jamaica, where he had found it. A few years later Mr. P. H. Gosse obtained an imperfect skin (without skull) which he sent to the British Museum, where it was described by Dr. Gray in the Proceedings of the Zoological Society of London, 1849. Dr. Gray gave it then no name, probably by reason of its imperfect characters. Later—in 1850—(Catalogue of Mammals in the British Museum) he described this same specimen as *Phoca tropicalis*, and afterwards (Catalogue of Seals and Whales, 1866) as *Monachus tropicalis*. But so imperfect was the specimen on which the description was founded, and the animal itself was so little known, that even its generic relations were in doubt, and its reference to the genus Monachus was considered provisional. From thence on to the present, rumours of the existence of this seal have been not unfrequent, but nothing seemed trustworthy and positive, and no specimens were obtained, if we except a young skin, without bones or skull, which came from Cuba to the National Museum at Washington, in 1884, without any indication as to locality.

It has long seemed to the writer—as, doubtless, to many others—that the certain presence in our waters of so important a mammal lying *perdu* in regions which our naturalist collectors are yearly visiting, was the opprobrium of American zoologists. We made inquiries, and collected notes from many sources, which showed clearly that this seal existed at isolated points—on small islands and keys—not only in the Caribbean and among the Bahamas, but also in the Gulf of Mexico. Last summer, while on a visit to the western shore of the Gulf of Mexico, we were so fortunate as to *locate* this seal with much certainty. This was upon the Triangles (Los Triangulos), three little keys, hardly above the water-level at high tide, and lying some 100 miles north-west off the Campeachy coast, in latitude N. 20° 50′, and longitude W. 92° 10′. Following this clue, my son, Mr. Henry L. Ward, last December visited the Triangles in company and partnership with Señor F. Ferrari Perez, naturalist of the Mexican Geographical and Exploring Expedition. His hunt was highly successful, and he has during the present month returned with nearly twenty specimens—skeletons and skins of all ages, from a suckling to the fully adult male, 7 feet in length. This ample material has just been carefully studied by Prof. J. A. Allen, the well-known zoologist, and author of the "Monograph of North American Pinnipeds." Prof. Allen has given a preliminary notice of the specimens in *Science*, January 14, 1887, and promises an elaborate account, with plates, in an early issue of the Bulletin of the American Museum of Natural History, New York.

It is a fact of rather peculiar interest that this, the first large mammal ever discovered in America, should, by the strange mishaps of natural history collecting, be the very last one to become known satisfactorily to science. HENRY A. WARD

Rochester, N.Y., January 30

An Abnormal *Hirudo medicinalis*

WHILST dissecting the leech in the class of practical zoology, one of my students directed my attention to an apparent abnormality in the specimen which it fell to his lot to dissect. On careful examination it was found that the vesicula seminalis of the right side had moved forwards into the fifth somite, and there opened into the base of a second and fully-developed penis, which opened to the exterior on the second annulus of the fifth somite. From the vas deferens, however, there passed off to the normal penis a duct which had on it a swelling corresponding in position to the vesicula seminalis, which had been moved forwards. The various parts on the left side, as well as the female organs, were quite normal.

R. J. HARVEY GIBSON

Biological Laboratory, University College, Liverpool,
February 14

Instinctive Action

SOME years ago I was about to drown a terrier pup of about a month old. I held it across the palm of my open hand over a large tub of water. It lay quite still on my hand as I gently lowered it. When within 4 inches of the surface, but not yet touching the water, it deliberately began, and continued as long as I held it there, the paddling motion with its feet peculiar to dogs when swimming, and quite unlike that of walking, although I am perfectly certain this puppy had never seen or touched water before. We know almost all animals swim when first placed in water, but how could this puppy know before it touched the water that this peculiar action would be necessary? Has a similar case been observed by any of your readers?

Birmingham, February 17 D. W. C.

THE RELATIONS BETWEEN GEOLOGY AND THE MINERALOGICAL SCIENCES[1]

I.

THE realm of Nature has been recognised from time immemorial as consisting of three kingdoms: dealing with the affairs of these three kingdoms, respectively, there have grown up side by side three departments of natural knowledge—zoology, botany, and mineralogy. But in recent years new and, I cannot help thinking, regrettable relations have sprung up between these sister sciences. Zoology and botany, having developed a method, a classification, and a nomenclature, based on common principles, have been drawn together by bonds so close and firm that many regard them as indissolubly one—the science of biology. Mineralogy, thus isolated, has been driven to seek new and unnatural alliances—with chemistry, with physics, or with the mathematical sciences. For my own part I confess that I regard this threatened "Repeal of the Union" of the natural sciences as alike a misfortune and a mistake.

It is sometimes assumed that the objects dealt with by zoology and botany are so different in their essential characters from those treated of by mineralogy, that the science of "organic" Nature must always follow a different path from that pursued by the science of "inorganic" Nature. The structures commonly known as *organic*, and the processes usually called *vital*, are asserted to be so entirely different, alike in their origin and in their essence, from anything existing in the mineral kingdom, as to warrant the establishment and perpetuation of a fundamental distinction between the sciences dealing with "living" and "non-living" matter respectively.

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[1] Address to the Geological Society at the Anniversary Meeting on February 18, by the President, Prof. John W. Judd, F.R.S.

In the year 1854 a very acute thinker, who at one time occupied this chair, made a serious attempt to formulate the distinctions which are supposed to divide living from non-living matter; but at a subsequent date, admitting with characteristic candour that he had altogether outgrown these ideas, Prof. Huxley argued, with great skill and cogency, that "vitality" is merely a general term for a set of purely physical processes, differing only in their complexity from those to which "inorganic" matter is subject.

It is a circumstance of no small significance that no definition of *life* which has yet been proposed will exclude the kind of processes which we can now show to be continually going on in mineral bodies. "Life," said the late George Henry Lewes, "is a series of definite and successive changes, both of structure and composition, which take place in an individual without changing its identity." Mr. Herbert Spencer prefers to define life as "the definite combination of heterogeneous changes, both simultaneous and successive, in correspondence with external co-existences and sequences."

If either or both of these definitions of life be accepted as satisfactory, then, as I hope to demonstrate to you, the minerals which build up the crust of our globe unquestionably live. At all events I am confident of being able to show that "in correspondence with external co-existences and sequences," or, in other words, as the conditions to which they are subjected vary, they undergo "a series of definite and successive changes, both in structure and composition, without losing their identity."

It may seem paradoxical, but it is nevertheless true, that the "vitality" of minerals—I really do not know what other term to use to convey my meaning—is much greater than that of plants, and, *a fortiori*, than that of animals; and this is the direct and necessary consequence of their less complex and more stable chemical constitution.

The zoologist regards as a case of remarkable vitality the recovery of snails which had been long affixed to a museum-tablet, upon their immersion in warm water. The botanist cites the germination of seeds taken from ancient Egyptian tombs as a striking illustration of how long life may remain dormant in the vegetable world. Let us now turn to the mineral kingdom. A quartz-crystal develops to certain dimensions, in accordance with the natural laws of its being, and when the necessary conditions of growth cease to environ it, its increase is arrested. But the crystal still retains its "vitality," that is, the power of further development which is dependent on its particular "organisation" or molecular structure. We may destroy that "organisation" and the "vitality" which is dependent upon it in a single instant, by subjecting the crystal to the action of hydrofluoric acid or of an oxyhydrogen flame. But unless its "organisation" and "vitality" be thus brutally stamped out, the crystal and, indeed, every fragment of it retains, not the "promise" only, but the very "potency of life." It may be worn by wind and wave into a rounded and polished sand-grain; it may be washed from the beds of one formation, to form part of the materials of a new one, and this process may be repeated again and again; but after countless wanderings and unnumbered "accidents by flood and field," extending over millions on millions of years, let but the necessary conditions of growth again environ it, and the battered and worn fragment will re-develop, in all their exquisite symmetry, its polished facets, it will assume once more the form of a quartz-crystal, having at least as much claim to *identity* with the original one, as a man has with the baby from which he has grown.

"Life!" "Vitality!" These terms are but convenient cloaks of our ignorance of the somewhat complicated series of purely physical processes going on within plants and animals. "Organisation!" Why should the term be applied to the molecular structure of an *Amœba* or a yeast-cell, and refused to that of a crystal? But even if we choose to insist on such distinctions as these, must we also make them a basis on which to establish our classification of the sciences?

Unquestionably there are differences between the cycles of change which take place in animals, plants, and minerals respectively. As the animal differs from the plant in not being able to build up its tissues from the simple compounds of the mineral kingdom, so both animals and plants differ from minerals in their power of growth by intussusception.

But perhaps the most striking difference of all between the "vital" processes in animals, plants, and minerals, is found in the *rate* at which they take place. Animals, in consequence of the instability of their chemical constitution, are distinguished by an almost ceaseless activity and a consequent brevity of existence. Plants, in the slower rate at which their vital processes take place, bridge over to some extent the tremendous gap between animals and minerals. In these last the vital processes are so prolonged in their manifestations, owing to the stability of their chemical composition, and they are not unfrequently interrupted by such enormous intervals of time, that they are only recognised by the geologist.

The cycles of change which take place in an ephemera are rapid indeed as compared with those going on in the oak-tree, among the branches of which it lives; but in the rocks among which the oak thrusts its rootlets, other processes are going on compared with which the life of the oak-tree is as "fast" as that of the ephemera compared with its own.

Nevertheless the three forms of life seem to start pretty much on a level. A solution of nitre in which crystallites are uniting, in obedience to the laws of polarity, to build up crystals, with their regular forms, their molecular structure, and their powers of further development; a solution of sugar, in which the cell of a yeast-plant is living and growing; and a third liquid with floating vegetable particles, in which an *Amœba* is increasing and multiplying;—these three may surely be compared with one another, however unlike may appear to be the higher developments in the three kingdoms to which they respectively belong.

I do not, of course, for one moment wish to suggest that it is practicable, or even desirable, to attempt an extension of the conventional use of the terms "life" and "organisation." But I do think that it is of the first importance that we should clearly recognise the fact that the distinctions between living and non-living matter are not essential and fundamental ones; that cycles of change exactly similar in almost every respect to those occurring in the animal and vegetable kingdoms are equally characteristic of the mineral kingdom; though, in the latter, they are more difficult to follow on account of the extreme slowness with which they take place.

When this great truth is fully recognised, the separation of the biological and the mineralogical sciences will be at an end, and mineralogy will begin to profit by that revolution in thought and in method which has already done so much for her sister sciences.

The temporary divorce between biology and mineralogy has arisen, not from any inherent differences between their aims, their methods, or the objects of which they treat, but from the circumstance that, while the former has in the last half century advanced with the stride of a giant, the latter has during the same period tottered on with the feeble steps of infancy. Mineralogy is still in the "pupa-stage" of its development; it is a classificatory science, with its methods imperfect, its taxonomy undeveloped, and its very notation undefined. Its cultivators, absorbed in the Sisyphean task of establishing new species and varieties, too often treat their science, with all its glorious possibilities, as though it were but akin to postage-stamp lore!

How is it, we may profitably ask, that the biological sciences have made such prodigious advances, while the mineralogical ones have lagged so far behind? We must ascribe the result, I believe, to two causes:—

In the first place, improvements in the construction of the microscope, and more especially the perfecting of methods of study by means of thin sections, have immeasurably enlarged the biologist's field of observation; histology and the cell-theory, embryology with all its suggestiveness, and many important branches of physiological research, must have languished, if, indeed, they could ever have seen the light, but for aid afforded by the microscopical methods of inquiry.

In the second place, the growth of geological and palæontological knowledge has been the leading factor in that profound revolution in biological ideas which, sweeping before it the superstition of fixity of species, has endowed this branch of natural science with the transforming conception of evolution.

Now these two causes, which have done so much for biology, are already working out the regeneration of mineralogy; and I doubt not that the fruits brought forth by the latter science will be equally satisfactory with those of the former.

The application of the microscope to the study of minerals has proved less easy than in the case of animal and vegetable structures. More than a century ago, it is true, several French geologists employed the method of crushing a rock, and of picking out from its powder the several minerals of which it was composed, for microscopic study; and in 1816, Cordier endeavoured, by systematising the methods followed by his predecessors, Daubenton, Dolomieu, Fleurian, and others, to elaborate a scheme for the mineralogical analysis of rocks by the aid of the microscope. In recent years the French geologists, with MM. Fouqué and Michel Lévy at their head, have shown how, by the employment of the electro-magnet, of fluids of high density, and of various chemical reagents, this work of isolating the several minerals of a rock for microscopic study or chemical analysis may be greatly facilitated.

But the great drawback to this method of microscopic study of rocks, as devised in France, was found in the circumstance that it began by destroying the rock as a whole, and hopelessly obliterating the relations of its mineralogical constituents. Delesse and other observers, it is true, succeeded in obviating this difficulty, to some extent, by studying the structure of rocks as seen in polished surfaces under the microscope by reflected light.

The greatest step in advance in connection with the microscopic study of rocks was undoubtedly made, however, when it was shown that transparent sections of minerals, rocks, and fossils can be prepared, comparable to those so constantly employed by biologists in their researches. William Nicol, of Edinburgh, was the first to discover, in the year 1827, how the mechanical difficulties in the way of the preparation of such sections could best be surmounted;. while Mr. Sorby, in a memorable communication to this Society, in 1858, showed us the first-fruits of the wonderful harvest of results to be obtained by the employment of this method.

But if the birthplace of the one method of microscopic study of rocks was France, and of the other Britain, it must be confessed that a large part of the merit of developing and improving these methods of inquiry is due to the Germans. To the labours of the numerous, patient, and accurate students in that country must be ascribed much of the perfection to which the methods of microscopic mineralogy have now attained; though we must not forget in this connection many most valuable contributions to the study from Scandinavia, Holland, Italy, and the United States.

As in the case of biology the results attained by the geologist have been the means of awakening new interests and inspiring a new philosophy, so in the case of mineralogy other problems have been suggested, and entirely fresh conceptions of the scope of the science have followed from the development of geological thought. We are thus led to regard minerals, not simply as a set of curious illustrations of mathematical and chemical laws, but as important factors in the evolution of the globe. Mineral collections in the past have resembled greenhouses, wherein only beautiful, though often abnormal growths are admitted; but in the future they will be like the herbaria of the botanist, where mere beauties of form and colouring are subordinated to the illustration of natural relationships and to the elucidation of the great problems of origin and development. Far be it from me to undervalue those wonderful crystals, the choice flowers of the mineral kingdom, which adorn our museums; but as there are many plants of extreme scientific interest which happen to possess only inconspicuous flowers, so there are not a few microscopic minerals, the study of which may lead us to the recognition of some of the most important laws of the mineral world.

I believe that what geology has already done for biology she is now accomplishing for mineralogy; it may, indeed, be instructive to point out how, in every one of its departments, the employment of microscopic methods and the suggestion of new lines of thought is causing mineralogy to develop in just the same directions as biology has already taken before her. In this way we may perhaps best convince ourselves that mineralogy is once more asserting her position in the family of the natural sciences.

Every natural-history science presents us with four distinct classes of problems. With respect to the objects of our study, we may make inquiries concerning their forms, their actions, their relations, and their origin. The answers to the first class of questions constitute *Morphology*, to the second *Physiology*, to the third *Chorology* or *Distribution*, and to the fourth *Ætiology*. The great problems of the mineral world, as I shall proceed to show, fall under precisely the same categories; and we may perhaps gather some useful hints by a comparison between the immature results of the mineralogist in each of these departments and those more perfect ones which have been attained by the botanist and zoologist.

The morphology of minerals was for a long time studied to the exclusion of all other branches of the science; for the problems connected with form and structure were those which naturally first attracted the students of the "inorganic" world.

Few generalisations of science are so beautiful, and at at the same time so suggestive, as those which have been arrived at by a discussion of the accurate measurements of crystal-angles. The constancy, within certain narrow limits, of corresponding angles, amid the almost infinite diversity of form assumed by crystals of the same mineral, is not less striking than the simplicity of the mathematical laws by which all these varied forms can be shown to be related to one another.

But the study of the morphology of minerals, which cannot be carried beyond a certain point by the aid of the goniometer, is capable of being pushed infinitely farther when we investigate the internal structure of their crystals, as illustrated by their optical and other physical properties. Not only do we find the minutest details of their external form to be correlated with peculiarities of molecular structure, as revealed by their action on a beam of polarised light, but delicate differences in internal organisation which the goniometer is powerless to detect, become clearly manifested under the searching tests of optical analysis. For the mineralogist, indeed, the polariscope with its accessories has supplemented the goniometer, in the same way as the spectroscope has the balance of the chemist.

What has been stated concerning the optical characters of minerals is equally true of their other physical properties; for the researches of recent years have shown all these properties to be intimately related to the symmetry of the crystal in which they are displayed. In every crystal, the faces of each group bearing the same relations to its axes exhibit characteristic peculiarities in their lustre, in their hardness, and in the manner in which they are acted upon by solvents; and these serve to distinguish such groups of faces from others in the same crystal having different relations to its axes. The elasticity of crystals, their power of conducting heat and electricity, and their phosphorescent, electric, or magnetic properties, whether natural or induced, are all manifested in varying degrees along certain directions which can be shown to be related to the particular symmetry of the crystal. And the more carefully we study both the forms and the physical properties of minerals, the more are we impressed by the conviction that the most intimate relations exist between these characters and the chemical composition of the minerals.

The phenomenon of "plesiomorphism," as Miller proposed to call it, that is, the slight variation in the angular measurements of crystals in the same species or group, when any of the constituents are replaced by vicarious or isomorphous representatives, very strikingly illustrates this conclusion. And the exact study of the optical properties of minerals shows that the slightest variation in the relative proportions of these vicarious constituents makes its influence felt by changes in their colour, in their pleochroism, in the nature and amount of their double refraction, in the position of their optic axes, and, indeed, in the whole assemblage of the properties of the crystal.

To the admirable investigations of Tschermak on the feldspars, the amphiboles and pyroxenes, the micas, and other groups of minerals, we are largely indebted for the establishment of this conclusion; while Doelter, Max Schuster, and other mineralogists, have contributed many striking observations which serve to extend and fortify it.

The application of the microscope to the study of the internal structure of minerals—their histology—has led to the recognition of many beautiful and unsuspected phenomena. Examined in this way, the seemingly homogeneous masses exhibit many interesting intergrowths and inclosures; and the study of these, as shown by Sorby, Vogelsang, Renard, and Noel Hartley, may serve to throw new and important light upon the conditions under which the crystals were originally developed. Cavities containing carbonic acid and other liquids, with bubbles in constant and, seemingly, spontaneous movement, serve to awaken the interest of the naturalist not less powerfully than the mysterious creeping of protoplasm in the hair of a nettle, or the dance of blood-corpuscles in the foot of a frog!

Others among these histological peculiarities of crystals must be regarded as having a pathological significance; they are abnormal developments resulting from unfavourable conditions to which the crystals may have been subjected during their growth, or in the course of their long and chequered existence.

The variability exhibited in crystals of the same mineral is sometimes very startling. In addition to the varieties due to the combinations of many different forms, or to the excessive development of certain phases at the expense of others, we have the complicated and diversified structures built up by twinning according to different laws. Again, by oscillatory tendencies in the same crystal towards the assumption of different forms, or by the existence of causes calculated to interfere with the free action of the crystallising forces, we may obtain varieties with curiously curved or striated faces. Not unfrequently large quantities of extraneous materials, solid, liquid, or gaseous, may be caught up in the crystal during its growth, and these foreign substances may be so far affected by the polar forces operating around them as to be made to assume definite and symmetrical positions within the crystal.

Even in the case of minerals of identical chemical composition and similar crystalline form, marked variations in physical properties may result from differences in the conditions under which they have originated. In lustre, density, and other characters, adularia differs from sanidine, and elæolite from nepheline. Dr. Arthur Becker has shown that quartz exhibits marked variations in its specific gravity, according to the particular conditions under which it has been formed.

There is one kind of morphological variability in minerals which has during recent years attracted a great amount of attention, and excited much discussion among mineralogists. Soon after his memorable discovery of the relations between the crystalline forms of minerals and their optical properties, Brewster detected certain apparent exceptions to his important generalisation; and since his day many additions to these curious anomalies in the optical behaviour of minerals have been made by other observers. So greatly, indeed, have these been multiplied in recent years, that it is doubtful whether any mineral crystallising in the cubic, the tetragonal, or the hexagonal system could be cited in which the optical properties are precisely what they ought to be according to theory; and similar anomalies are also found in crystals possessing lower degrees of symmetry.

The attempts which have been made by some crystallographers to account for these optical anomalies in crystals, by assuming that they possess only a pseudosymmetry, the result of very complicated twinning, ingenious as they undoubtedly are, remind one of the wonderful addition of eccentrics and epicycles by which astronomers so long sought to maintain the credit of the Ptolemaic theory. But as, in the latter case, complexities and difficulties alike vanished when the centre of the system was shifted from the earth to the sun, so have the discoveries of Klein, Rosenbusch, and others removed the necessity for the painfully elaborate crystallographic hypotheses to which we have referred.

Most mineralogists will now be prepared to admit, as the result of these researches, that the perfection alike of form and of optical properties which characterises a crystal when first formed, is liable to slight modification, as the conditions of temperature and pressure under which it exists vary. In consequence of this, almost all natural crystals are found, when we study them with sufficient care, to exhibit slight but very striking and significant differences in form and optical behaviour from what they ought theoretically to possess.

While our knowledge of the ordinary mineral varieties promises to be vastly extended by the improvements which have been made in the methods of optical and chemical diagnosis under the microscope, there is, at the same time, reason to hope that the relationship of these numerous varieties will, by the same means, be made more clearly apparent. As the existence of well-defined natural groups of minerals becomes more clearly established, through the study of interesting though inconspicuous links, we shall obtain a basis for a much-needed reform in mineral taxonomy and nomenclature.

The more carefully we pursue our researches among the diversified forms of the mineral world, the more are we impressed by the conviction that each mineral, like each plant or animal, possesses its own individuality. Nature does not make *facsimiles* in the mineral, any more than in the vegetable or the animal kingdoms. All the sciences of Nature must be content to recognise individuals as the only real entities, and to accept species, like genera, families, and orders, as convenient but purely artificial conceptions.

The geological study of minerals leads us to regard each specimen that we examine as possessing a distinguishing combination of properties, some of which are impressed upon it by causes operating when it came into being, while others are no less clearly the result of the long series of vicissitudes through which it has since passed.

Of all the branches of mineral morphology there is none from the study of which the geologist has gained more in the past, or from which he has greater reason to look for future aid, than that of the embryology of crystals.

In the year 1840 Link showed that the first step in the formation of crystals in a solution consists in the separation of minute spherules of supersaturated liquid in the mass; and subsequently Harting in Holland, and Rainey and Ord in this country, obtained a number of interesting experimental results, by allowing crystallisation to take place slowly in mixtures of crystalloids and colloids.

Valuable contributions to the same subject were made by Frankenheim, Leydolt, and others; but it is to Hermann Vogelsang that we owe the greatest and most important contributions to mineral-embryology. By the ingenious device of adding viscous substances to solutions in which crystallisation was going on, he succeeded in so far retarding the rate at which the operation took place as to be able to study its several stages. He thus showed how the minute "globulites," gathering themselves into nebulous masses or ranging themselves according to mathematical laws, gradually build up skeleton-crystals, by the clothing of which the perfect structures arise.

Since the early and regretted death of Vogelsang, the subject of the development of crystals from their embryos, the so-called *crystallites*, has been successfully prosecuted by Behrens, Otto Lehmann, Wichman, and other investigators.

Now in all glasses—whether of natural or artificial origin—in which the process of primary devitrification is going on, we have examples of the growth of crystals in a viscous and retarding mass, and in these, as Leydolt, Zirkel, and Vogelsang clearly saw, admirable opportunities are afforded to us for studying the formation of crystallites, and the laws which govern the union and growth of these into crystals. Two years ago, my predecessor in this chair submitted to you the interesting results of his own researches upon the devitrification of artificial glasses and slags; and the subject has since been pursued by Velain in France, and by Hermann and Rutley in this country.

The igneous rocks supply us with admirable opportunities for studying mineral embryology. In the same rock-mass we may sometimes find every possible graduation, from an almost perfect glass to a holocrystalline aggregate. By the study with the microscope of the several transitions in different parts of the mass, we obtain data for the most important conclusions concerning the phenomena of crystal-development.

There is another line of research in connection with mineral-embryology, which appears to be full of promise, and which has not yet received all the attention it deserves. In the "contact-zones" around great igneous intrusions, we find the curious so-called "spotted slates," which under the microscope are seen to contain nebulous patches, the mere ghostly presentments of crystals, struggling into being in the amorphous mass. The development of these nebulous masses into perfect crystals, exhibiting the characteristic external forms and optical properties of andalusite and kyanite, of garnet and epidote, of hornblende and mica, may be traced in some cases with the greatest facility.

More complicated still are the phenomena exhibited along the foliation-planes of the rocks, which have been made to flow in the act of mountain-making. There, as the old minerals are destroyed, new ones build themselves up from their elements. The study of all the steps of this process is an undoubtedly difficult one, but the results already obtained by Reusch, Lossen, Heim, and Lehmann, by Lapworth, Teall, Roland Irving, and Williams, lead us to look hopefully forward to the full solution of the grand but complicated problems of regional metamorphism.

The field of mineral-embryology is indeed a promising one, and its diligent cultivators may hope to gather a harvest no less rich than that which has been reaped by the workers in the same department of the biological sciences.

(*To be continued.*)

TABASHEER

I HAVE often wondered that this curious substance has never attracted more attention. But scanty references to it are to be found in books, and yet it seems to me that few more singular things are to be met with in the vegetable kingdom.

In Watts's "Dictionary of Chemistry" (vol. v. p. 653), exactly six lines are devoted to it. It is defined to be: "Hydrated silica, occurring in stony concretions in the joints of the bamboo. It resembles hydrophane, and when thrown upon water does not sink till completely saturated therewith." It is further stated to be the least refractive of all known solids, and an analysis by Rost von Tonningen of a specimen from Java gives a composition of 86·39 per cent. silica soluble in potash, 4·81 potash, 7·63 water, with traces of ferric oxide (to which I suppose its occasional yellowish colour to be due), lime, and organic matter.

There are several specimens in the Kew Museums, partly derived from the India Museum. All consist of small irregular angular fragments, varying from the size of a pea downwards, and opaque white in colour. It is obvious that these fragments are the debris of large masses.

Now, the presence of considerable solid masses of so inert a substance as hydrated silica in the plant-body is a striking fact. At first sight, one might compare it do the masses of calcium phosphate which form the endo-skeleton in the higher animals. These, however, serve an obvious mechanical purpose, which cannot be attributed to the lumps of tabasheer in the hollow joints of a bamboo. The presence of silica may sometimes serve an adaptive purpose, as in the beautiful enamelled surface of canes. And according to Dr. Vines ("Physiology of Plants," p. 21), "Struve found that it constitutes 99 per cent. of the dry epidermis of *Calamus Rotang*."[1]

In a few other groups of plants, such as *Equisetum* and the *Diatomaceæ*, it is a characteristic constituent. In all cases it principally occurs in the cell-wall (Vines, *l.c.* p. 137. This has suggested the highly ingenious speculation that, seeing the intimate chemical relationship which obtains between silicon and carbon, there might be a silicon-cellulose. I notice that Count Castracane, in his Report on the *Diatomaceæ* collected by the *Challenger*, speaks of its "having been already shown that silica is sometimes substituted for carbon in the formation of cellulose" (p. 7). Judging from ash-analyses it might be supposed that silica was an essential constituent of gramineous plants. But by the method of water-culture Sachs has found that maize, for example, will grow with only a trace of silica. I must confess to ignorance of all that may have been done in the matter recently. But Ladenburg thought, and I think with reason, that the indifference of the plant to silica was a strong argument for a silicon-cellulose in which silicon might or might not with equal physiological convenience play the part of one or more atoms of carbon. Fascinating as this hypothesis is, I am bound to say that the prolonged investigation which he devoted to the question is on the whole adverse to the idea of silicon playing any part of the kind.

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[1] Sachs remarks ("Text-book," second edition, p. 700) that silica accumulates chiefly in the tissues exposed to evaporation, though this clearly does not apply to the case of diatoms.

It still remains then an unsolved problem why, when no adaptive end is involved, plants should take up such relatively enormous quantities of silica. The case of the frustules of *Diatomaceæ* is peculiar, as there the silicious wall is apparently a continuous plate of inorganic matter capable of resisting without impairment treatment by the most destructive and disintegrating agencies known Yet Castracane adduces evidence to show that such walls can grow; and as this can only be by interstitial growth, a molecular constitution is implied quite different from anything physical, and precisely similar to that of a cellulose membrane. He quotes, indeed, von Mohl for the opinion that the wall is not simply inorganic, "but only an organic membrane which is impregnated with silex."

Now, in the case of tabasheer, it is quite evident that the plant takes up an amount of silica beyond its powers to use, and so it is exuded into the hollow cavities of the bamboo stem. I do not mind confessing that, in so far as I had reflected on the matter at all, I had pictured to myself this as taking place by some process of secretion, so that the mass of tabasheer ultimately accumulated from successive portions of thrown-off silica. I was obliged, however, to give a little more serious thought to the matter when Prof. Cohn, of Breslau, wrote to me that he proposed to investigate the whole subject, and asked for help in the way of specimens and information. It then struck me what a very singular thing the phenomenon of the occurrence of tabasheer really was. I set to work to hunt up in the literature of Indian botany some rational account of the matter. The only ray of light I got was from the "Forest Flora of North-West and Central India," by Dr. Brandis, late Inspector-General of Forests to the Government of India. Everyone who knows Dr. Brandis knows that he gave to administration the energy he would more willingly have devoted to scientific pursuits. I was not at all surprised to find, therefore, modestly hidden in his book (p. 566) the key to the riddle. He says: It is not at all impossible that the well-known silicious deposit (*tabasheer*) which is found in the joints of this and other species [*Bambusa arundinacea*] may be the residuum of the fluid which often fills the joints." I communicated this to Prof. Cohn, and he was good enough to tell me that he quite agreed that this was the correct explanation. I at the same time wrote to Dr. King, the distinguished Superintendent of the Royal Botanic Garden, Calcutta, to know if it were possible to procure specimens of tabasheer *in situ*, as we possessed in our Museum nothing but broken fragments. I extract from several letters he has written me the following particulars:—"January 11. I have inquired of several old workers as to the situation tabasheer occupies. They all say it is found either on the *floor* of the joint, or if (as is so often the case in *B. Tulda*) the stem leans over, it is also found on the lower wall. It is never found on the roof of a joint. . . . . Tabasheer is not common in bamboo grown near Calcutta. And, besides, it is apt to be forced out of its natural position by the forced used in breaking a joint open. There is no external mark by which a tabasheer-bearing joint can be recognised prior to being opened." "January 18. I have got a specimen of tabasheer *in situ* for you. It concretes *as a jelly*, and is now being carefully dried off."

I think that these extracts (in which the italics are mine) fully confirm the explanation as far as I know first put out by Dr. Brandis. The rapidity of growth of a bamboo shoot is well known to be enormous. The root-pressure is probably equally great. The joints, at first solid, become hollow by the rending apart of the internal tissues, and water containing silica in solution is poured out into the cavities so formed. When the foliage is developed, transpiration is active: the water taken up from the ground is rapidly got rid of; not merely is the root-pressure compensated, but the water poured out into the joints is re-absorbed. It is not easy to see why the silica should not be always taken with it, as in the vast majority of cases it no doubt is. But in the cases in which it is left behind it has apparently simply undergone a process of dialysis. The determining causes of the occasional deposit of tabasheer are, I think, still obscure. But, as Prof. Cohn intends to investigate the subject, I think we may pretty confidently look forward to an exhaustive explanation.

It is a well-known fact that a large proportion of the ash-constituents of plants may have but little significance in their nutrition. The chemical constitution of plants, as far as their ash is concerned, to a large extent varies with the nature of the soil in which they are grown. It is quite certain that they will in consequence take up a vastly larger proportion of certain constituents than they can turn to any physiological account. Tabasheer is a striking instance of one such case. The calcareous masses found in the wood of many Indian trees mentioned in NATURE, vol. xxi. p. 376, affords another.

W. T. THISELTON DYER

ON THE EARLIER TRIPOS OF THE UNIVERSITY OF CAMBRIDGE

I HAVE read with great interest the papers by Mr. Glaisher in NATURE of December 2, 18, and 30, 1886 (pp. 101, 153, and 199), entitled "The Mathematical Tripos." Through the period common to Mr. Glaisher's notes and my recollections, I believe that we are strictly in agreement. I am able, however, to supply some little histories (I wish these had been more numerous and more certain) relating to transactions several years earlier than those known, personally, to Mr. Glaisher, and I am desirous that their memory should not be totally lost. There are now few persons, perhaps none, whose recollections of the University of Cambridge and of Trinity College go so far back as my own.

I first advert to the official course of undergraduates' life.

Shortly after introduction to the College in the October Term of 1819, I attended, with all other freshmen, in the Senate House or in the College Hall (I believe the latter) to take the oaths of allegiance and supremacy. With great ardour I renounced the "damnable doctrine" that the Pope of Rome could absolve subjects from their allegiance, with several similar declarations; and I also disclaimed all connection with other Universities and Colleges, and in particular with Wolsey's College at Ipswich. I believe (but have no certain knowledge) that these puerilities terminated a few years afterwards.

The undergraduates were arranged in "sides," divided under the official tutors under whom they were entered in the College Lists. There were then two "sides"; subsequently there were three. The lectures on each side were held in the College rooms of the tutor or his assistant tutor. The lectures consisted, naturally, in proposals of theorems and problems (in writing) and oral discussion of the answers in a friendly style.

The annual College examinations of the undergraduates of all sides (collected), of each year of undergraduateship, were held in the College Hall at the practical termination of the May Term. The order of merit in each year, as determined by these examinations, was published by lists of names suspended in the College Hall. Small sums of money, to be expended in honorary prizes, were assigned to the First Class of each year.

In the third year of undergraduateship arrived the time of "keeping Acts and Opponencies." These, as Mr. Glaisher has explained, were formerly the only public exhibitions of students' merits in the University; and, possibly, were still considered in the University as more important than would be gathered from Mr. Glaisher's account. The three Opponents met to take tea and to arrange their arguments; the Act also was invited, with an intimation that he was not to stop long. I have seen the "Schools," in which the disputations were held, quite filled with undergraduates of all Colleges, who came to listen to the disputations, or rather quibbles, held in the Latin language, of the argumentative quarrel. If my memory is correct, each of the undergraduates (selected, I believe, by the Moderators) appeared twice in the character of "Act" (asserting the correctness of some doctrine in the printed books), and twice or more in the character of "Opponent" (denying that correctness). The President of the School was one of the Moderators. The assertion of the Moderator that the argument failed was given by the words "Probes aliter." The discussion was usually closed by a complimentary address of the Moderator, as, for instance, "Magno ingenio argumenta tua et construxisti et defendisti." I do not think that the form lasted many years after this time.

At length came the October Term, the last term (the tenth) for undergraduates, of which I remember only one characteristic, namely, that in the College Hall a separate dinner-table was established for the "Questionists" (as those were called who were to proceed to the B.A. degree in January). To this table all Questionists were removed from whatever tables they previously occupied. Among others, the "Scholars" of the College (Trinity) were removed from their table, where they had formed agreeable acquaintances, to a collection of strangers, naturally disagreeable to the "Scholars." We much disliked this change. I think that in this term the character of the College lectures was changed almost entirely to problems and questions; some of them in the evening, in the College rooms of one of the Fellows.

At length arrived the Monday morning on which the examination for the B.A. degree was to begin. A breakfast was given by the "Father" of the College (one of the Fellows of the College) in the College Combination Room, and then we were all marched in a body to the Senate House and placed in the hands of the Moderators. How the "candidates for honours" were separated from the οἱ πολλοὶ I do not know. I presume that the Acts and Opponencies had something to do with it. The honour-candidates were divided into six groups; and of these Nos. 1 and 2 (united), Nos. 3 and 4 (united), and Nos. 5 and 6 (united), received the questions of one Moderator. No. 1, Nos. 2 and 3 (united), Nos. 4 and 5 (united), and No. 6, received those of the other Moderator. The Moderators were reversed on alternate days. There were no printed question-papers: each examiner had his bound manuscript papers of questions, and he read out his first question; each of the examinees who thought himself able proceeded to write out his answer, and then orally called out "Done." The Moderator, as soon as he thought proper, proceeded with another question. I think there was only one course of questions on each day (terminating before 3 o'clock, for the Hall dinner).

The examination continued to Friday mid-day. On Saturday morning, about 8 o'clock, the list of honours (manuscript) was nailed on the door of the Senate House.

The ceremonies and customs of conferring degrees in the middle of the day, I believe, have not been altered. The Vice-Chancellor was seated in the centre of the Senate House. The Father of that College of which the Senior Wrangler was a member led him to the Vice-Chancellor. The roar of acclamation from the undergraduates in the galleries of the Senate House, to welcome a favourite Senior Wrangler, will not be forgotten by one who has heard it. The Father presented him with the words: "Dignissime Domine, Domine Procancellarie, et tota Universitas, præsento vobis hunc juvenem, quem scio tam moribus quam doctrinâ esse idoneum ad respondendum Quæstioni; idque tibi fide meâ præsto, totique Universitati." The candidate knelt before the Vice-Chancellor, who pressed the candidate's hands between his own, and answered: "Auctoritate mihi commissâ, admitto te ad respondendum Quæstioni, in nomine Patris, et Filii, et Spiritûs Sancti." I am not able to say how much of this was repeated for each candidate. Then followed some petty quibbles with some Master of Arts concerning questions which nobody professed to understand, but which were inessential. The undergraduate gown was then changed for a B.A. gown.

On a certain day following, at a Congregation of the Senate, the list of names of those who were thus admitted was read to the Vice-Chancellor, who (as I understood) solemnly recognised the rights of the first to the privileges of Bachelor of Arts, and to each of those following only repeated the words "et ei," finally declaring that they were "actualiter in Artibus Baccalaureos."

It is evident that there must have been some relation between the various ranks which no longer exists; and, in particular, that the *Quæstio* was once important, and is now totally lost. And connections existed between the Colleges and the University which can scarcely be traced at the present day.

I now advert to the mathematical subjects of study and examination.

In the October Term, 1819, the only books on pure mathematics were: Euclid generally; "Algebra," by Dr. Wood (formerly Tutor, but, in 1819, Master, of St. John's College), Vince's "Fluxions" and Dealtry's "Fluxions," Woodhouse's and other Trigonometries. Not a whisper passed through the University generally on the subject of differential calculus, although some papers (subsequently much valued) on that subject had been written by Mr. Woodhouse, Fellow of Caius College; but their style was repulsive, and they never took hold of the University. Whewell's "Mechanics" (1819) contains a few and easy applications of the differential calculus. The books on applied mathematics were: Wood's "Mechanics," Whewell's "Mechanics," Wood's "Optics," Vince's "Hydrostatics," Vince's "Astronomy," Woodhouse's "Plane Astronomy" (perhaps rather later); the first book of Newton's "Principia." I do not remember any others. These works were undoubtedly able; and I do not conceal my opinion that for the great proportion of University students going into active life books constructed on the principles of those which I have cited were more useful than those exclusively founded on the more modern system. For those students who aimed at the mastery of results—more difficult and (in the intellectual sense) more important—the older books were quite insufficient.

More aspiring students read, and generally with much care, several parts of Newton's "Principia," Book I. and also Book III. (perhaps the noblest example of the geometrical form of cosmical theory that the world has seen). I remember some questions from Book III. proposed in the Senate House Examination, 1823.

In the October Term, 1819, I went up to the University. The works of Wood and Vince, which I have mentioned, still occupied the lecture-rooms. But a great change was in preparation for the University course of mathematics. During the great Continental war, the intercourse between men of science in England and in France had been most insignificant. But in the autumn of 1819 three members of the Senate (John Herschel, George Peacock, and Charles Babbage) had entered into the mathematical society of Paris, and brought away some of the works on pure mathematics (especially those of Lacroix) and on mechanics (principally Poisson's). In 1820 they made a translation of Lacroix's "Differential Calculus," and they prepared a volume of "Examples of the Differential and Integral Calculus." These were extensively studied; but the form of the College examinations or the University examinations was not, I think, influenced by them in the winter 1820-21 or the two following terms. But in the winter 1821-22 Peacock was one of the Moderators; and in the Senate-House Examination, January 1822, he boldly proposed a paper of important questions entirely in the differential calculus. This was considered as establishing the new system in the University. In January 1823 I think the two systems were mingled. Though I was myself subject to that examination, I grieve to say that I have forgotten many of the details, except that I well remember that some of the questions referred to Newton, Book III., on the lunar theory. To these I have already alluded.

No other work occurs to me as worthy of mention, except Woodhouse's "Lunar Theory," entirely founded on the differential calculus. The style of this book was not attractive, and it was very little read.

From this time to the times of which Mr. Glaisher treats, there were successive books on the new system, but none, I think, which can be cited as producing a marked effect in the University. G. B. AIRY

NOTES

THE University of Bologna has decided to celebrate its eight hundredth anniversary in the spring of 1888. The exact date of its foundation cannot, indeed, be determined, but all authorities on the subject agree that an important school was established at Bologna in the eleventh century. Afterwards the University took a great place as the chief centre for the study of jurisprudence, and there also anatomy was for the first time scientifically studied. The foremost Universities of Europe and America will no doubt be glad to take this opportunity of testifying their respect for so ancient and famous an institution.

AT their meeting on February 3 the President and Council of the Royal Society adopted the following resolutions concerning the publication of the Philosophical Transactions:—That the Transactions be published in two independent series, one (*a*) containing those papers which are of a mathematical or physical character, the other (*b*) those of a biological character; that the papers in each series form a yearly volume, paged continuously (though issued in parts if the Secretaries find it convenient), but that each paper be also published separately in paper covers as soon as it is ready for publication; and that Fellows have the option of receiving one or both of the yearly volumes, or, should they prefer it, each separate paper of either or both series, or the whole of one series and any separate paper of the other series, immediately on publication. These resolutions are to take effect with the volume for the present year.

A COMMITTEE, lately appointed by the College of Physicians of Edinburgh, has reported in favour of the establishment and maintenance, by the College, of a laboratory for the prosecution of original research. The Committee proposes that the College shall vote from its capital 1000*l.* for the establishment of this laboratory, and, year by year, a sum not exceeding a third of the clear surplus of annual income over annual expenditure for its maintenance, including the payment of salaries The superintendent, it is thought, should devote his time wholly to the direction and prosecution of scientific research in the laboratory; and it is suggested that an assistant experienced in microscopic work should be engaged to reside on the premises. The Committee is of opinion that the laboratory should be open without fee to Fellows of the College, to members, and to any licentiate, medical man, or investigator who may, by testimonial or otherwise, be able to show that he is a fit person to use the laboratory for purposes of scientific research. Moreover, the Committee recommends that, if there should be sufficient funds after payment of expenses, a medal and money-prize (not exceeding twenty guineas) should be offered for original work. It is understood that this admirable scheme will be adopted, and we may hope that the example set by the Edinburgh College will soon be followed by the English College of Surgeons and College of Physicians.

A CORRESPONDENT writes to us from Tashkend that on November 29 last, at 9.12 a.m., a violent shock of earthquake was felt there, accompanied by a great noise like thunder. The people were much frightened, and the majority of the buildings were more or less injured. Light shocks were felt also on December 3, and on January 9 and 16.

ON the 15th of January last there was in some parts of Japan the most severe earthquake that has been experienced there since February 22, 1880. It fetched down a number of chimneys and parts of roofs in Yokohama, but in Tokio (eighteen miles distant) it only broke a few vases and created alarm. The important and peculiar feature of the disturbance was that it had a long period and large amplitude. At the Imperial Observatory at Tokio, where a number of "Gray-Milne seismographs" are employed, the pointers of the instrument were seen to move for nearly ten minutes. We learn from the *Japan Gazette* that the disturbance was felt at Tokio at 6h. 51m. 59s. p.m., commencing in a series of small waves. The greatest horizontal movement was 19·2 mm. (about ¾ inch). The time taken to describe the largest wave was 2·3 seconds. The vertical motion had a range of 5·5 mm. (about ¼ inch), and its period was ·8 seconds. Altogether there were 60 distinct waves, and the maximum velocity with which the earth moved to and fro was 26 mm., or about 1 inch per second. At Tokio people felt the motion as if they had been on a slowly-moving floating pier, and in many cases it provoked a sensation of nausea. The general distribution of destruction at Yokohama was similar to that which took place in 1880. The *Japan Gazette*, however, points out that many chimneys standing on ground which in 1880 suffered severely were this time uninjured. This "anomaly," it thinks, may be explained by the fact that the owners of these chimneys took advantage of the experience they gained in 1880, and rebuilt their chimneys with a special view to their safety.

THE Right Hon. G. Sclater Booth, M.P., has accepted the Presidentship of the Congress of the Sanitary Institute of Great Britain, to be held at Bolton in September next.

ON March 6 a century will have elapsed since the birth of the celebrated optician, Joseph von Fraunhofer, at Straubing, in Lower Bavaria. Preparations are being made in Munich for the due celebration of the day.

IN the eleventh annual Report of the President of the Johns Hopkins University, Baltimore, Dr. Gilman is able to give a very satisfactory account of the progress made by the institution since its establishment in 1876. Much of its success, he thinks, is due to the system of Fellowships. Every year twenty young men who have given evidence of their attainments and of intellectual promise are selected by the authorities as Fellows, and are encouraged to devote all their time to the study of some branch of knowledge in which they have already shown proficiency. During the first ten years this honour has been bestowed upon 130 persons. Their names and the stations to which they have been called have been frequently printed, and Dr. Gilman says a scrutiny of the list will show that it contains the names of many excellent scholars. While resident at the University, the Fellows are recognised as holding an intermediate position between the Faculty and the great body of pupils. They are efficient members of the various literary and scientific associations, and occasionally give lectures on topics which they have specially studied.

A NEW quarterly journal, to be entitled the *American Journal of Psychology*, will soon be issued. Dr. G. Stanley Hall, Professor of Psychology and Pedagogics in the Johns Hopkins University, will be the editor. The *Journal* will contain original contributions of a scientific character; and articles of unusual importance will be translated from other languages, or even reprinted from other English and American publications, in full or in abstract, if not generally accessible. An attempt will be made in each number to give a conspectus of the more important psychological literature of the preceding three months, and to review significant books, bad as well as good.

A SCIENTIFIC and industrial Exhibition will be opened at Ekaterineburg in May next. The mining industries of the Ural Mountains will be well represented. Special interest will attach to the department of ethnography, as it has been arranged that there shall be in the Exhibition a number of families belonging to semi-barbarous tribes of the Ural Mountains and Siberia. Their dwellings will be exactly like those in which they usually live, and they will have with them the weapons and implements used by them in hunting and fishing. Another important element will be a collection of ancient objects in stone, bone, clay, and metal, found in Siberia and among the Ural Mountains. These objects have never before been publicly exhibited.

A CAUCASIAN Agricultural Exhibition will be held next year in Tiflis. Products of the animal, vegetable, and mineral kingdoms will be exhibited.

FISH-HATCHING operations have begun at the Buckland Museum, where consignments of trout ova, presented by Sir James Maitland, have been laid down in the incubating apparatus used by the late Mr. Frank Buckland. The system upon which the ova are hatched at the Museum is that known as the "overflow," the water passing over the eggs, which are placed in slate boxes lined with gravel. The new system is called the "underflow," the water passing underneath the eggs, which are deposited in perforated zinc trays without gravel. Much diversity of opinion exists as to the efficacy of the two systems.

THE other day some workmen, while removing brickwork that had surrounded a tank in the late South Kensington Aquarium, found ten eels secreted in a crevice of the masonry, which was perfectly dry. The tank had been removed eighteen days before, when the Aquarium was dismantled, so that the fish must have been without water during the whole of that time. When placed in water they appeared to have been in no way injured by their terrestrial experience.

IN the debate on the appropriations for the support of the U.S. Coast Survey, the Senate, according to *Science*, pared the items down in a parsimonious spirit. Afterwards the Senate Appropriations Committee addressed a letter to the Secretary of the Treasury, inquiring whether the estimates as submitted by the Superintendent of the Coast Survey were satisfactory to that Department. The answer was that they were perfectly satisfactory, and a communication from the Superintendent of the Coast Survey was submitted, showing the reasons for each item of expenditure, and the present condition of the service. "From these communications," wrote the Secretary, "it appears that the estimates made provision for the efficient and economical prosecution of the Survey during the ensuing year; it also appears that the provision made by the House Bill will not secure such results. Consequently the arrangement made is not satisfactory to this Department."

WE learn from Italy that the idea of boring a tunnel between the peninsula and Sicily has been revived. The estimated cost is said to be seventy-one millions of francs, and the time required for completing the work would be from four to six years. It is stated that the depth of the sea is 160 metres.

THE number of foreigners at present residing in France, and settled there, is 1,115,214, against 37,103,689 Frenchmen. The parts of France in which the foreigners are most numerous are of course the frontier departments, those of the Nord, Alpes Maritimes, Var, Bouches du Rhône, &c. In the Seine Department there are 213,529 foreigners; in the Nord 305,524, most of whom are Belgians.

OYSTER-PRODUCTION, although carried on to a large extent in France, is not yet a profitable investment. The reason is that the rates for transportation from the oyster-beds are too high. In Auray, for instance, oysters are worth nine francs per thousand; in Paris they cost more than fifty francs. An attempt is being made to secure transportation at less cost.

A MOVEMENT is on foot in the North Sea towns of Germany for promoting oyster-culture along the coast, supported by Government grants. At present there are fifty-one banks in the North Sea, viz. twenty-six at Fanö, Romö, and Sylt, and twenty-five at Föhr, Amrum, and Hallingerne. In the Baltic, on the other hand, all attempts at oyster-culture have failed. "Holstein" or "Flensburg" oysters—considered the best in Germany—are really English or Dutch. All the German oyster-banks are the property of the State, and leased to private individuals.

WE regret to announce the death of Dr. Walfried Marx, Professor of Descriptive Geometry at the Technical High School at Munich. He died on February 10.

THE first International Horticultural Exhibition will be held at Dresden on May 7.

HERE is a case in which even a little knowledge of physics would not have been out of place. A man was summoned for making use of the communication between passengers and guard without reasonable and sufficient cause. Being in a third-class compartment alone, he was frightened by the singing noise of his foot-warmer, caused by the contraction of metal due to the reduction of temperature. Thinking it an infernal machine, he immediately threw it out of the window, and, not content with this drastic proceeding, he incontinently proceeded to stop the train by using the excellent mode of communicating with the guard and engine-driver which exists on the South-Eastern Railway. A little more science and a little less energy would have saved everybody a little trouble, but as his *bona fides* was as obvious as his ignorance, the magistrate dismissed him with a caution. The magistrate might, perhaps, have done some good if he had told the man what happens when an ordinary kettle is filled with ordinary water and placed on an ordinary fire.

IN a recent number, *Science* referred to a supposition that "it is change of diet which is the most potent remote cause of consumption among the Indians." Mr. H. C. Wyman, of Detroit, writes to that journal that, in his opinion, another cause is change change of dress. "If," says Mr. Wyman, "a live rabbit be dipped in a solution of glue, so as to cover its body with a coating impervious to air, it is surprising how quickly the frequency of the respiratory movements increases, showing that the work of the lungs is increased by depriving the skin of free access to the air. The process of civilisation has a somewhat similar effect upon the Indian, though to a less degree. One of the first lessons in the effort to civilise him teaches him to envelop himself in clothing of a kind that tends to impede and impair the normal action of the skin, the pores of which are organs of excretion—a mechanism by which morbid and waste material may be thrown out of the system. Deprived of the assistance afforded under previous conditions by the skin, the work of the lungs is greatly increased, rendering them peculiarly susceptible to bronchitis and pneumonia—ailments which are commonly the forerunners of consumption. If we accept the theory of Koch, they make the lungs a suitable habitation for the *Bacillus tuberculosis*." Mr. Wyman contends that, in the case of civilised races, the liability to consumption from overworked lungs has been tempered by hundreds of generations of ancestors habituated to the use of clothing.

In the Report of the U.S. Geological Survey on the mineral resources of the United States for 1885, it is stated that the total mineral product is valued at 428,521,356 dollars, an increase of 15,306,608 dollars over 1884. Among seventy mineral substances cited, coal is the most important, showing a total value of 159,019,596 dollars. An increase is shown in the production of coke, natural gas, gold, silver, copper, zinc, quicksilver, nickel, aluminium, lime, salt, cement, phosphate rock, manganese, and cobalt oxide, while the production of coal, petroleum, pig-iron, lead, precious stones, and mineral waters decreased. According to the Report, it is probable that the total output of 1886 was much greater than that of 1885, and even larger than that of 1882.

We have received the *Annuaire* for 1887 of the Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. It contains a full account of the organisation of the Academy, and of the means by which it seeks to encourage science, literature, and art. Among the biographical notices are articles on the late François Lenormant and Edouard Morren.

An illustrated work, entitled "Les Civilisations de l'Inde," by Dr. Gustave Le Bon, is being issued in weekly parts by Firmin-Didot et Cie. Dr. Gustave Le Bon is the author of a work on "La Civilisation des Arabes."

The Calendar of the Imperial University of Japan for the current year, which we have just received, deserves special mention, for it is the first that has been issued since the amalgamation of the well-known College of Engineering and the University of Tokio into the single institution which forms the new University. This incorporation was made the occasion for several organic changes, one of which is the almost total elimination of Europeans from the teaching staff, their places being taken by Japanese. To understand the full extent of the change in this respect, it is necessary to remember that five or six years ago all the professors in the College of Engineering, and nearly all in the University, were Europeans. An examination of the Calendar shows how the new University stands in this respect. By the Imperial decree, which is the charter of the institution, the Council of Professors regulate the studies and generally look after the interests of the University and each of its Colleges. There is not a single Western amongst the nine Councillors. In the Law College, three out of thirteen professors and lecturers are Westerns; in the Medical Faculty, out of thirty-four professors and assistants, there are three Europeans; there are three Europeans amongst twenty-four professors and assistants in the Faculty of Engineering; two in twenty-six in Science; and three in nineteen in Literature. It is probably not too much to say that, where one European is teaching now, there were six Europeans five years ago. It was only to be expected that ultimately the Japanese would have their own men ready to take the places vacated by Europeans. They had at vast expense sent abroad large numbers of youths to be educated for the various professions in Europe and America, who, on their return, were competent to teach their countrymen; and in looking down the lists of the Japanese professors we see that most of them have foreign degrees and other qualifications. They come from almost every German, French, British, and American University, and in some instances have taken high honours. They may therefore be presumed to be competent for the work which they have undertaken, and there is no special reason to believe that the step taken by the Minister of Public Instruction in placing the higher education of the country in the hands of his countrymen is premature. Be this as it may, it is clear that the day of Europeans in Japanese education is past, and this fact is only emphasised by the few familiar names amongst a host of unfamiliar ones in the list of the University. Moreover, even where there are one or two foreign professors, the direction is all in Japanese hands. The Director and the chief Professor in every Faculty are Japanese, so that if the individuals are fewer in number, the functions and status of each one remaining have also diminished. Time alone will show whether the experiment—for undoubtedly the Imperial University is at present in the experimental stage—will be successful or not. He would be a bold man who, for example, prophesied that the Faculty of Engineering in the new University will maintain the high position won for the old Imperial College of Engineering in the world of science by a body of brilliant European professors, some of whom are now in the front rank at home. In the Calendar there is certainly evidence of much activity. An Astronomical Observatory has been fitted up for the instruction of the students; there is also a Seismological Observatory, with horizontal pendulum and vertical-motion seismographs. By the aid of a complete set of these instruments now in the Observatory, it is possible to measure earth-movements of different grades of magnitude. In addition, a system of telegraphic communication with different parts of the city extends the area of observation. A Botanic Garden and a small Marine Biological Laboratory are likewise attached to the Science Department.

The additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♀) from West Africa, presented by Mr. Julius Wilson; a Secretary Vulture (*Serpentarius reptilivorus*) from South Africa, presented by Capt. Larmer, s.s. *Trojan*; a Crowned Hawk Eagle (*Spizaetus coronatus*) from Natal, presented by Colonel H. Bowker, F.Z.S.; a Spotted Eagle Owl (*Bubo maculosa*) from South Africa, presented by Mr. H. Justice; a Brazilian Hangnest (*Icterus jamaicai*) from Brazil, presented by Mr. W. G. Little Gilmour; two Crossbills (*Loxia curvirostris*), British, presented by Mr. W. H. St. Quintin; two White-fronted Lemurs (*Lemur albifrons*) from Madagascar, deposited; a —— Capuchin (*Cebus* ——) from South America; two Chimachima Milvagos (*Milvago chimachima*) from Brazil, purchased.

OUR ASTRONOMICAL COLUMN

The Binary Star δ Equulei.—This close and rapid binary, which was discovered in 1852 by M. O. Struve, and measured on a number of nights between that time and 1874, was supposed by its discoverer to have a period of between 6 and 7 years or one of about 13 years. More recently, Mr. Burnham, who observed the star between 1880 and 1883, concluded from an examination of all the measures that the period was about 10·8 years. It is therefore the most rapid binary now known. At the request of Prof. Glasenapp of St. Petersburg, Herr Wrublewsky (*Astron. Nachr.* No. 2771) has computed a set of elements from all the available observations. He finds the time of periastron passage to be 1892·03, with eccentricity = 0·2011, mean distance, = 0″·406 and period = 11·478 years. These elements do not, however, represent the observed position-angles satisfactorily, and it is desirable that the possessors of sufficiently powerful telescopes should pay some attention to this very interesting object, especially at the present time, when the components are about at their maximum distance apart. Herr Wrublewsky's orbit gives, for 1887·24, position-angle = 204°·9, and distance = 0″·48.

The Temple Observatory.—From the Report of Mr. Seabroke, honorary Curator of the Temple Observatory, Rugby, we learn that during 1886 Mr. Percy Smith continued the measurement of position-angles and distances of double stars, 88 sets of measures having been secured. These stars have been divided into three categories for future re-measurement, viz. rapid binaries, to be observed every year; slower binaries, to be observed every 4 years; and long-period binaries, to be observed every 10 years. Mr. Seabroke himself has continued the measurement of the motion of stars in the line of sight with the spectroscope on the reflector, and has completed 100 sets of measures. These observations, together with the corresponding ones for previous years, have been published in the January number of the *Monthly Notices*.

DISCOVERY OF A NEW COMET, 1887*d* (BARNARD 2).—A new comet was discovered on February 15 by Mr. E. E. Barnard, Nashville, Tennessee. It was very faint, and was moving rapidly in a north-westerly direction. At midnight (local time) its position was R.A. 8h. 4m., Decl. 16° 10′ S.

PROBABLE NEW VARIABLE.—We learn from Circular No. 15 of the Liverpool Astronomical Society, that Mr. Backhouse finds 28 Andromedæ to be probably variable within small limits. The observations yet obtained are insufficient to fix the period, which must, however, be short. It is possible that the star is of the Algol type.

NAMES OF MINOR PLANETS.—Herr J. Palisa has named Minor Planet No. 256 Walpurga.

BRIGHTNESS AND MASS OF BINARY STARS.—The current number of the *Observatory* contains an article on this subject by Mr. W. H. S. Monck, in which he attempts to deduce the relative brilliancy of those binaries for which the orbits are best determined. Assuming that the mass of the companion-star is very small as compared with that of its primary, he shows that the relative brilliancy of any two pairs of binaries may be found by the following formula:—

$$\frac{k_1}{k_2} = \left(\frac{I_1}{I_2}\right) \cdot \left(\frac{P_1}{P_2}\right)^{\frac{4}{3}} \cdot \left(\frac{a_2}{a_1}\right)^2,$$

where $I_1\,I_2$ stand for the total amount of light, as determined photometrically, which we receive from the two pairs respectively; $P_1\,P_2$ for their periods; and $a_1\,a_2$ for the angular radii of their orbits.

By, apparently, a printer's error, the index of $\left(\frac{P_1}{P_2}\right)$ is omitted in the formula in the *Observatory*. Adopting ξ Ursæ Majoris as his unit of comparison, Mr. Monck finds the brilliancy of γ Leonis 93·29; of Castor, 38·24; δ Cygni, 35·52; of Sirius, 7·17; 42 Comæ, 2·79; 6 (*p*) Eridani, 0·20; and 61 Cygni, 0·08. It is noteworthy that Prof. E. C. Pickering, in a paper which appeared in the Proceedings of the American Academy of Arts and Sciences, vol. viii. No. 1, obtained very similar results for many of the same stars, but by a somewhat different process. In both lists γ Leonis figures at the head, followed by Castor and δ Cygni, whilst the smallest values are found for *p* Eridani and 61 Cygni. The weak point in Mr. Monck's computation is the assumption that the mass of the smaller star is comparatively insensible; the near equality in magnitude of many of the binaries selected would seem to indicate that the assumption was not a safe one. Mr. Monck repeats Prof. Pickering's suggestion that series of careful measurements should be made between each component of the binary systems and some neighbouring stars, so that the ratio of the masses of the two components may be determined. It is to be hoped that some double-star observers may be induced to take up this interesting subject, now that attention has again been called to its importance. The research might also possibly supply us in some cases with a determination of the distance of the binary.

THE LIVERPOOL ASTRONOMICAL SOCIETY.—The Pernambuco branch of this Society now numbers more than eighty members, and has been accorded permission to elect a local executive. The Emperor of Brazil has been elected a member of the Society.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 FEBRUARY 27—MARCH 5

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 27

Sun rises, 6h. 52m.; souths, 12h. 12m. 56·0s.; sets, 17h. 34m.; decl. on meridian, 8° 21′ S.: Sidereal Time at Sunset, 4h. 3m.

Moon (at First Quarter March 3) rises, 8h. 50m.; souths, 15h. 30m.; sets, 22h. 21m.; decl. on meridian, 7° 9′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	... 7 17	... 13 13	... 19 9	... 1 36 S.
Venus ...	... 7 34	... 13 31	... 19 28	... 1 23 S.
Mars ...	... 7 19	... 13 1	... 18 43	... 4 11 S.
Jupiter ...	... 22 48*	... 3 49	... 8 50	... 12 8 S.
Saturn ...	... 12 30	... 20 39	... 4 48*	... 22 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

March	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
2 ...	Aldebaran	... 1	... 17 47	... 18 4	... 182 210
4 ...	130 Tauri ..	... 6	... 2 30	... 2 38	... 43 25

March	h.	
3 ...	3 ...	Mercury at least distance from the Sun.
5 ...	11 ...	Mercury at greatest elongation from the Sun, 18° east.
5 ...	14 ...	Saturn in conjunction with and 3° 29′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	... 81 16 N.	... Mar. 2,	19 57	*m*
Algol	3 0·8	... 40 31 N.	... Feb. 28,	2 54	*m*
			Mar. 2,	23 43	*m*
U Monocerotis ...	7 25·4	... 9 33 S.	... Feb. 28,		*m*
T Canis Minoris ...	7 27·7	... 11 59 N.	... ,, 28,		*M*
S Cancri	8 37·5	... 19 26 N.	... Mar. 2,	23 26	*m*
R Leonis	9 41·5	... 11 57 N.	... ,, 2,		*m*
U Virginis	12 45·4	... 6 10 N.	... ,, 5,		*m*
W Virginis	13 20·2	... 2 48 S.	... ,, 4,	0 0	*m*
S Boötis	14 19·3	... 54 20 N.	... ,, 2,		*M*
δ Libræ	14 54·9	... 8 4 S.	... ,, 3,	0 5	*m*
U Coronæ	15 13·6	... 32 4 N.	... Feb. 27,	21 2	*m*
U Ophiuchi	17 10·8	... 1 20 N.	... Mar. 3,	1 8	*m*
		and at intervals of		20 8	
W Sagittarii ...	17 57·8	... 29 35 S.	... Mar. 3,	4 0	*m*
R Scuti	18 41·5	... 5 50 S.	... ,, 3,		*M*
β Lyræ	18 45·9	... 33 14 N.	... ,, 5,	20 0	*M*
R Lyræ	18 51·9	... 43 48 N.	... Feb. 28,		*M*
δ Cephei	22 25·0	... 57 50 N.	... Mar. 2,	23 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers

Amongst the meteor-showers of the season are the two following:—Near δ Virginis, R.A. 192°, Decl. 1° N; near ξ Sagittarii, R.A. 280°, Decl. 17° S. The latter radiant gives very swift streak-bearing meteors.

GEOGRAPHICAL NOTES

TWO letters have been received in Vienna from Dr. O. Lenz, dated, one from Lake Tanganyika in September, and the other from the River Shiré in December. This indicates that the Austrian Expedition has taken an unexpected route to the east coast. When Lenz and his companions left Kasonge, on the Upper Congo, on June 30, they made for Tanganyika, arriving at Capt. Hore's station on the west shore on August 7. Crossing to Ujiji, Dr. Lenz found that it was impossible to proceed northwards to the Albert Nyanza and Emin Pasha, on account of the Arab raids and the state of things in Uganda. Instead, therefore, of proceeding eastwards to Zanzibar, he travelled, by land apparently, to the south end of Lake Tanganyika, along the Stevenson road to Lake Nyassa, down that lake to the Shiré, and thence by the Zambesi to Quillimane. The two letters will be published in the next number of the *Mitteilungen* of the Vienna Society, and will doubtless contain a good deal of information of interest.

TIPPOO TIP, about whom we have heard so much recently in connection with the Emin Pasha expedition, seems to be rather an intelligent man, and even finds time in the midst of his ivory raids to attend to the interests of science. He recently came upon a remarkable tribe on the Congo, to the north of Nyangwé, who do a great deal of work in copper, and whose inlaid work in that metal is of a highly artistic character. He sent several specimens to an English friend at Zanzibar, who has brought them with him to this country. Still more interesting is the discovery by Tippoo, among the same people, of what may be regarded as the first steps towards a currency. Spears are naturally among the most valuable articles which such a people possess, and, as a matter of fact, the value of everything is reckoned by them in terms of spears. Not only so, but they have actually reached the stage of a conventional currency. Enormous spear-heads of very thin copper are made, some six feet in length, which are passed from hand to hand, just as bank-notes are with us. These spears, for example, in the purchase of ivory, are valued at £200—their intrinsic value being probably not so many pence. We are glad to know that a specimen is likely to be deposited in the British Museum. Readers of Schweinfurth's "Heart of Africa" will remember that among the Niam-Niams hoes are used for a similar purpose, only after a reverse fashion; tiny hoes, what we should call mere toys, are in common use as money.

THE principal article in the new number of *Petermann's Mitteilungen* is a summary of the journey across Africa from Mossamedes to Quillimane, by the Portuguese travellers, MM. Capello and Ivens in 1884-85. The most valuable geographical work accomplished by the travellers was the exploration of the interesting region lying between the Upper Zambesi and Lake Bangweolo. The important north-east tributary of the Zambesi, the Kabompo, was traced to its source in the closest proximity to the sources of the Lualaba, one of the most important contributories to the Congo. From here a zigzag was made eastwards and southwards, across the head-waters of many affluents of the Zambesi, until that river was reached about 16° S. and 29° E. MM. Capello and Ivens took very numerous astronomical and meteorological observations during their journey, as well as observations for terrestrial magnetism. The complete narrative of the journey, with ample supply of maps and scientific appendixes, has just been published in Portuguese. The same number of the *Mitteilungen* contains a large collection of barometric data on the hypsometry of South America, mainly Peru and Bolivia.

PROF. L. BODIO sends to the *Bollettino* of the Italian Geographical Society for December 1886 an important paper on Italian emigration, which he divides into two categories—permanent and temporary. The latter, which is essentially of a periodical character, varies from 80,000 to 100,000 persons yearly, and consists chiefly of stonemasons, bricklayers, navvies, and other day-labourers from the northern provinces of Piedmont, Lombardy, and Venice, who seek casual employment on the public works in Austria, France, Germany, Switzerland, Corsica, and elsewhere. They generally leave their homes in the spring, returning with their earnings towards the close of autumn, and enjoy the reputation of sober, steady, intelligent workmen. The permanent movement, which alone constitutes emigration properly so called, has already risen during the last twenty years from less than 20,000 to about 80,000 annually, and is directed from the same northern provinces, and from Liguria and parts of Naples, almost exclusively to the Argentine States and some other parts of the New World. The emigrants, who sail either directly from Genoa, Naples, and Palermo, or from the French ports of Marseilles, Bordeaux, and Havre, comprise between 60 and 80 per cent. of male adults, the small minority consisting of women and children. They represent nearly all social conditions, the peasant class, however, largely predominating in South America. For the year 1885 the returns show 57,827 to the Argentine Republic; 15,485 to the United States; 12,311 to Brazil; and 1477 to Uruguay. The chief inducements to leave their native land and settle abroad appear to be poverty, the desire to better their fortunes, and the direct encouragement of friends and relatives who have prospered in their new homes across the Atlantic. Very few ever return to reside permanently in the mother country.

THE Germans are losing no time in making themselves acquainted with the section of New Guinea which they have annexed. The Empress Augusta River, close to the western boundary of the German territory, was recently navigated by Admiral von Schleinitz and Dr. Schrader, in the steamer *Ottilie*, for a distance of 224 miles. It being the dry season, the river was too shallow for further navigation by the steamer. The ship's steam-launch, however, proceeded 112 miles further, to a point situated in 4° 16′ S. and 141° 50′ E.; judging from the quantity of water in the river the voyage could have been continued 50 miles further, but fuel ran short. For over 200 miles from its mouth the river flows through extensive plains; then its course suddenly changes, and it assumes the character of a mountain stream, forcing its way through hills of gneiss, mica-slate, and quartz; but the velocity of its current remains uniform. The settlements on its banks were only found at long intervals.

ON THE CONSTITUTION OF THE NITROGENOUS ORGANIC MATTER OF SOILS

THE organic matter of soils, the residue of the limited oxidation of vegetable and animal matter, has appeared a subject so complex and obscure, and promising the investigator so little of definite result, that it has received but scanty attention. The researches made have been chiefly confined to a study of the non-nitrogenous humic acids, the nitrogenous organic bodies present in soil have been scarcely at all investigated. The agricultural chemist has indeed not unfrequently spoken and written as if such investigation was superfluous, holding that the nitrogenous organic bodies contained in humus were not capable of serving as food for farm crops until they had undergone a further change into ammonia, and finally into nitric acid. A valuable paper, "Sur les principes azotés de la terre végétale," by Berthelot and André, which appeared in the *Comptes rendus* of December 6, has called attention to this neglected subject, and has done much to clear up our ideas respecting the constitution of the nitrogenous organic matter contained in soils. Like many other epoch-making treatises, the paper in question brings forward facts which have, in part, been already established by earlier investigators; but in no earlier investigation, as far as I am aware, have the facts appeared in such a striking aspect, nor have the conclusions which flow from them been clearly set forth.

Berthelot and André conclude that the nitrogenous matter of soils is mainly composed of insoluble amides;[1] these amides are decomposable by the action of acids, alkalies, and to a less extent by water, into ammonia and soluble amides (amido-acids), in the same manner as other bodies of the same class with which the chemist is already quite familiar. The behaviour of soil towards hydrochloric acid furnishes the main facts on which the French chemists base their conclusions. They find that when a soil tolerably rich in nitrogen (0·174 per cent.) is treated with dilute hydrochloric acid, a quantity of ammonia is found in the solution, which is greater as the strength of the acid is increased, as the time of its action is lengthened, and especially as the temperature is raised; two hours' boiling produces, in fact, with various strengths of acid, four, five, and six times as much ammonia as five days' action in the cold. Besides ammonia, there is found in the acid solution a considerable quantity of some nitrogenous organic body, the amount of which rises and falls with the quantity of the ammonia. In cases in which the action of the acid was carried farthest, the nitrogen of the soluble organic body bore to the nitrogen of the ammonia a proportion of about 3 to 1. The extent to which the nitrogenous matter of the soil was attacked by the hydrochloric acid was very considerable; boiling 200 grammes of soil for two hours with 400 cubic centimetres of water, and 100 cubic centimetres of strong hydrochloric acid, resulted in the solution of 31·8 per cent. of the soil nitrogen, and the conversion of 7·1 per cent. of it into ammonia. The nature of the nitrogenous organic matter found in solution in the hydrochloric acid has apparently not been particularly investigated by Berthelot and André, but the whole reaction is so characteristic of the splitting up of an amide that their view of the constitution of this body becomes highly probable.

Investigations earlier than those of Berthelot had shown that hydrochloric acid dissolves nitrogenous matter from the soil. Loges has pointed out that this solution contains a nitrogenous body precipitable by phospho-tungstic acid. The nitrogen and carbon in this precipitate had a relation of about 1 to 6·2. My own experiments show that a nitrogenous body precipitable by phospho-tungstic acid is also extracted from soil by a cold solution of potassium carbonate. We may hope that, before long, further light will be thrown on the constitution of these bodies.

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[1] The presence of amides in soil was long ago inferred by S. W. Johnson ("How Crops Feed," p. 247), from the reactions of soil with which chemists were then acquainted.

The action of alkalies on soil is quite in accordance with the assumption of the amide nature of its nitrogenous compounds. Boussingault long ago showed that the agricultural operation of liming a soil caused the production of ammonia. It has recently been shown by Baumann, and others, that a solution of soda, even in the cold, develops a notable amount of ammonia in soil, while at a high temperature the action becomes very considerable.

Nor are facts wanting which seem to exhibit the actual synthesis of amides from ammonia and humic acids. Knop long ago observed that when peat was treated with ammonia the ammonia disappeared, and could no longer be detected. Joulie found, in his experiments on the changes which take place in farmyard manure, that when finely divided straw, horse-dung, and ammoniacal urine of known composition were mixed, and allowed to ferment, a great disappearance of ammonia took place, accompanied by a gain of 35 to 63 per cent. in the organic nitrogen. The ammonia had in this case clearly united with some of the organic compounds present.

The view of the constitution of the nitrogenous matter of the soil which has been now brought forward will, we think, prove fruitful: it throws much light on the chemical changes within the soil; it has also possibly important bearings on plant-nutrition. That the acid sap contained in roots is capable of rendering soluble, and thus effecting the assimilation of various mineral matters with which they come in contact, is admitted to be a fact by physiologists. May it not equally follow that the insoluble amides of the soil are also attacked by the acid root-sap? We know not yet the properties of the soluble amides which result from the action of acids on the insoluble amides of the soil; but if they are diffusible through a membrane, they must enter the plant, and it is certainly very probable that they would then be found capable of taking part in plant-nutrition. A reaction of the kind we have supposed between the root and the soil would probably take place to a very different extent with different plants, much depending on the character of the root-sap. When the subject has been more fully investigated, it may perhaps be found that we have in this action of the roots an explanation of those obscure cases of plant-nutrition which at present puzzle the agricultural chemist. R. WARINGTON

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following is the speech delivered by the Public Orator, Dr. Sandys, in presenting for the honorary degree of Doctor of Science, Prof. Alexander Agassiz, Curator of the Museum of Zoology, Harvard College, Massachusetts:—

Cum Collegio Harvardiano antiquitus consociati, nuper vetera amicitiae iura auspiciis optimis renovavimus; litteris datis acceptisque trans maria lata dextras iunximus; legatis denique insignibus missis, ludis illis saecularibus, etiam absentes, velut praesentes interfuimus. Hodie vero e Collegii illius professoribus unum revera praesentem videmus, virum et suo et patris et Collegii sui nomine nobis dilectum. Donec Alpium inter culmina ingentes illae glaciei moles desuper paullatim descendunt, tam diu patris illius nomen superstes vivet, qui, in Republica non magna natus, Rempublicam maximam gloriae suae fecit participem, expertus scilicet vetera illa verba quam vera essent:—

"Omne solum forti patria est, ut piscibus aequor,
Ut volucri vacuo quicquid in orbe patet."

Filii vero famam patre tanto non indignam, quibus potissimum verbis exsequi potero? Utinam tu mihi hodie adesses:—

O testudinis aureae
Dulcem quae strepitum, Pieri, temperas;
O mutis quoque piscibus
Donatura cycni, si libeat, sonum.

Atqui Musa illa vocata non audit; rogata tacet; virumque praeconio altiore dignum sermone pedestri laudandum relinquit. Ergo, utcunque possumus, virum libenter laudamus, qui, cum ingenii sui ope aeris thesaurum ingentem invenisset, Academiam suam divitiarum suarum amplitudine ornavit, iudice me (insusurrare mihi videtur Horatius) iudice me, "non sordidus auctor naturae verique." Quid autem de vivario illo dicam, aequoris Atlantici prope marginem ulteriorem condito, ubi maris immensi miracula minutissima ab hoc viro accuratissime examinantur, ubi oceani ipsius e penetralibus profundis rerum naturae veritas ipsa audacter extorquetur? Satis erit hodie de veritate illa dicere quod olim de Romanorum virtute dictum est:—

"Merses profundo; pulchrior evenit."

Duco ad vos marinae praesertim zoologiae indagatorem indefessum, ALEXANDRUM AGASSIZ.

SCIENTIFIC SERIALS

THE *Quarterly Journal of Microscopial Science*, January.—The anatomy of the Madreporian coral Fungia, by G. C. Bourne (plates xxiii. to xxv.). During a visit to Diego Garcia (an atoll lying in 7° 13′ S. lat., 72° 23′ E. long.) which extended from the middle of September 1885 to the middle of January 1886, the author was able to collect and preserve a large number of specimens of *Fungia dentata*. These Fungiæ were very abundant within the lagoon, where at low spring tides they could be collected by scores from depths of from three to ten feet: a prolonged search failed to secure any specimens under two inches in diameter, or an example of the nurse-stock. It is suggested that the time of the year was the cause of this; the depth of the water in which the search had to be made was also unfortunate for such investigations. The name "mesoglœa," suggested by Prof. Lankester, is used to denote the supporting lamina of Cœlenterata: the only seeming objection to the name is that it is the name of a well-known genus of Algæ.—On some points in the development of *Petromyzon fluviatilis*, by Arthur E. Shipley (plates xxvi. to xxix.). The material was obtained by fertilising the eggs of the ripe female Lampern, hatching the larvæ out, and rearing them in confinement. The summary is too long for abstracting, but it may be mentioned that the early development of the skeleton is described up to the stage where Prof. Parker commenced his researches.—The ammoniacal decomposition of urine, by Dr. W. R. Smith (plate xxx.). Records a series of observations proving that the ammoniacal decomposition of urine is brought about by the presence of a Micrococcus which differs from that described by Prof. W. Leube, inasmuch as it liquefies gelatine. Though about twenty different organisms were isolated from one sample of healthy urine, only this one acted so.—Notes on Echinoderm morphology, No. 10; on the supposed presence of symbiotic Algæ in *Antedon rosacea*, by P. Herbert Carpenter (plate xxx.). Discusses the views of Vogt and Yung as to the Sacculi of Antedon being symbiotic Algæ, and considers these views as certainly not proven; an opinion which Perrier seems by intuition to have already ascribed to him.—The function of nettle-cells, by Dr. R. von Lendenfeld (plate xxx.). The plasmotic contractile coat of the cnidoblast is incited to action by the cnidocil: the animal can control this action.—Some new methods of using the aniline dyes for staining Bacteria, by E. H. Hankin. Illustrations of the structure and life-history of *Phytophthora infestans*, by Prof. H. Marshall Ward (plates xxxi. and xxxii.).—On the formation and liberation of the zoospores in the Saprolegnieæ, by Dr. Marcus M. Hartog.

THE *Journal of Botany* for January is chiefly occupied by a biographical notice of the late Dr. H. F. Hance, of Whampoa.—In the number for February, Dr. Richard Spruce describes and figures a Hepatica from Killarney new to science, to which he gives the name *Lejeunea Holtii*; Mr. Alfred Fryer continues his notes on the genus *Potamogeton*; and Mr. J. G. Baker commences a synopsis of the six genera *Sodiroa*, *Caraguata*, *Schlumbergeria*, *Guzmannia*, *Catopsis*, and *Tillandsia*, which make up the tribe Tillandsieæ of the natural order Bromeliaceæ.

Bulletin de l'Académie Royale de Belgique, December 1886.—Determination of the parallax relative to the larger member of the double star Σ 1516 of Struve, by L. de Ball. From previous observations the chief star of this group appeared to have a proper movement in a straight line independently of its companion, with which it had no physical connection. By means of a Cointe refractor the author has followed the relative displacements of the two stars, and has determined a periodicity, the effect of the relative parallax, which he finds to be

$$0''{\cdot}091 \pm 0''{\cdot}013,$$

and the distances

$$0''{\cdot}112 \pm 0''{\cdot}010.$$

From these elements he determines an absolute parallax 0″·104, with a mean error 0″·008, corresponding to a distance which light would take 31 years to traverse.—Note on the transparency of platina mirrors, by Edm. Van Aubel. His further researches confirm the author's previous conclusions regarding the false transparency of these mirrors, the light passing, not through the metal itself, but through the interstices left between the particles deposited on the plates as prepared by Paul Lohmann, of Berlin. —On the instability of equilibrium of the surface-layer of a liquid, second part, by G. Van der Mensbrugghe. The points here dealt with are: (1) the existence of a surface-tension proper to each liquid according to a given inner temperature; (2) the existence of a contractile or expansive force on the surface of a liquid in contact with a solid; (3) tension of a surface common to two liquids not intermingling.—On the valency of an atom of carbon, by Louis Henry. A method is proposed for determining the relative value of the four unities of its chemical action.—On the physiology of the heart of the dog, by Léon Fredericq. The author explains the nature of the contraction of the ventricles, the idio-muscular contraction of the cardiac muscle, the nervous system of the heart, its isolated circulation, and the circulation in the pulmonary artery.—The Neanderthal or Canstadt race in Belgium, by MM. Traipont and Lohest. The authors describe what appears to be the most important anthropological find ever made in Belgium. It consists of two more or less perfect human skeletons discovered by them in association with the remains of *Rhinoceros tichorinus*, *Elephas primigenius*, the cave hyæna, and other extinct animals in the undisturbed Lower Quaternary deposits of a limestone cave at Spy, on the banks of the Orneau, in the province of Namur. The human remains, which came to light during the summer of last year, present remarkable points of resemblance with those of the oldest yet discovered Palæolithic race, as represented by the Neanderthal and Canstadt skulls. The relationship is so close that the strikingly simian features of these skulls, hitherto regarded as possibly aberrant or pathological, would appear to be perfectly normal, and characteristic of the oldest known human inhabitants of Western and Central Europe. One of the skulls of the Spy men is decidedly platidolichocephalic (long and low), with cephalic index 70; the other is sub-platidolichocephalic, with index 74·80. The frontal bone is also very low, narrow, and retreating, and the upper alveolar process highly prognathous, while the chin is but slightly developed, receding more rapidly than that of even the lowest Papuan type.

Engler's Botanische Jahrbücher, Achter Band, 1 Heft.—The latest botanical discoveries in the tombs of Egypt, by G. Schweinfurth. This article, with an appendix dated October 1886, contains an enumeration and description of vegetable remains found in tombs at Dra-Abu'n-Negga. Though the specimens were often in bad preservation, the author has been able to recognise some fifty species of plants from tombs dating from various periods, both very ancient and comparatively modern; among others the garlic (*Allium sativum*), which, with leeks and onions, is mentioned in Numbers, chap. xi.—The next two articles by Alfredo Cogniaux and Dr. F. W. Klatt contain descriptions of the Melastomaceæ, Cucurbitaceæ, and Compositæ collected by Lehmann in Guatemala, Costa Rica, and Columbia.—On the family of the Lactoridaceæ, by A. Engler. The genus Lactoris has been placed by various authors in the Magnoliaceæ, Dilleniaceæ, and Piperaceæ (Saururaceæ). On anatomical as well as other grounds the author rejects the affinity to the Saururaceæ and Dilleniaceæ, and concludes that Lactoris is to be regarded as representing a family (Lactoridaceæ) closely allied to the Magnoliaceæ.—On Didymia, a new genus of Cyperaceæ, by Dr. R. A. Philippi (with one plate).—Contributions to the flora of the Congo district collected by Dr. Naumann on the expedition of H.M.S. *Gazelle*, prepared by A. Engler.—Then follow abstracts of papers published elsewhere.—At the conclusion of this number is a formal offer of prizes for monographs of the genera Ranunculus and Draba, and for a critical revision of the fossil forms of Quercus.

Achter Band, 2 Heft.—Dr. R. A. Philippi, on the Chilian species of Polyachyrus, a genus of Compositæ belonging exclusively to Chili and Peru. The author distinguishes the species according to the characters of the leaves, and illustrates his paper with a plate.—Hepaticæ Africanæ, by F. Stephani (one plate). This is a description of two collections of Hepaticæ: the one, made by F. A. Moller, from the Island of St. Thomas, consists of thirty-four species, of which twenty are new; the other, by W. Mönkemeyer, about the mouth of the Niger, consists of sixteen species, of which eight are new.—The Hepaticæ of the Peninsula of Alaska, prepared by F. Stephani, comprise four new species, three of which are figured on plate iii. The fourth (*Frullania chilcootiensis*) is extremely small, only a few millimetres in length, and is found hidden in the roughnesses in the bark of the birch.—Comparative anatomy of the leaf of the family Olacineæ, by E. Edelhoff. This is a laborious investigation of minute details of the anatomy of the leaf, the outcome of which is apparently no new view as to the grouping of the members of the family, but rather the recognition of microscopic diagnostic characters.—Dr. Gürich, on the botanical results of the expedition of Flegel to the Niger-Binué.—Note on a recently disclosed Pliocene flora in the neighbourhood of Frankfurt/a/M., by Dr. H. Th. Geyler.—Abstracts of papers published elsewhere.

Nuovo Giornale Botanico Italiano, January 1887.—Signor A. Piccone continues his observations on the part played by phytophagous fishes in the dissemination of Algæ. The fish which appears to be by far the most effective in this direction in *Box Salpa*, L.—Dr. F. Tassi contributes an elaborate paper on anæsthesia and poisoning in plants. Among the general conclusions at which the author has arrived, the more important are that there exists in certain plants a property analogous to that which in animals is variously denominated irritability, contractility, excitability, &c., but that this property is located in no special organ, but originates in the protoplasm. Some substances which produce anæsthetic or poisonous effects in animals are in no way injurious to plants.

Rendiconti della R. Accademia dei Lincei, December 1886.—Researches on the nature of malaria, carried out by Dr. Bernardo Schiavuzzi in Pola, Istria. The results of these experiments show the constant presence of a Bacillus, morphologically identical with that already described by Klebs and Tommasi-Crudeli, in the malarious districts of Pola, and its absence from the healthy localities. This Bacillus, artificially cultivated and inoculated on rabbits, develops fevers showing all the characteristics of swamp-fever, while in the infected animals the red corpuscles of the blood undergo the same alterations as Marchiafava and Celli have shown to be characteristic of malarious infection. These alterations, however, are attributed by Dr. Schiavuzzi, not to the presence of a parasitic animal which has never yet been detected either in the air or in the soil of the infected districts, but to a deterioration of the blood-corpuscles directly or indirectly caused by the action of a pathological ferment of quite a different nature. He accordingly concludes that the *Bacillus malariæ* described by Klebs and Tommasi-Crudeli in 1879 is the true cause of marsh-fever.—On the objective spectroscope, by L. Respighi. The author claims the honour of having first introduced and applied to stellar spectroscopy the improved form of this instrument, as now generally used by spectroscopists. Although the important modification made by him is commonly attributed to Secchi, he shows conclusively that it had been adopted and successfully employed by him fully nine months before its application by Secchi in November 1869. An account of his first experiments with the perfected instrument appeared in the Atti of the Academy for May 20, 1869. The modification in question consists in replacing the large prism of Fraunhofer's instrument by one with a small refrangent angle, by means of which may be obtained perfectly distinct and well-defined spectra of the smaller stars.

Rendiconti del Reale Istituto Lombardo, January 13.—Annual Report on the progress of the mathematical and natural sciences, presented by the Secretary, S. Ferrini. In this general survey of work done by members of the Istituto, special reference was made to E. G. Cantoni's memoir on the phenomenon of dew, showing that Aitken's observations have been confirmed by the results obtained in Italy by Fusinieri, Melloni, and Cantoni himself, in opposition to the generally admitted hypothesis of Wells; to C. Poloni's experimental researches on the permanent magnetism of steel at various temperatures, formulating the law of variations caused by changes of temperature, and on his new method for measuring the absolute thermic conductivity of metallic wires; and to Giacomo Cattaneo's studies on the formation of gastric and intestinal glands in the embryo of *Salmo salar*.

Rivista Scientifico-Industriale, January 15.—Electricity developed with the formation of fogs and clouds, by Prof. Luigi Palmieri. Some electric phenomena recently observed at the meteorological stations of Naples and Vesuvius are appealed to in confirmation of the author's view that strong electric tensions in clear skies constantly indicate the near approach of clouds fogs, and even rain. Hence the strong tensions of atmospheric electricity so frequently signalled from the New York Observatory some days before the arrival of storms and wet weather on the west coast of Europe. During thirty-six years of constant study, the author has recorded thousands of similar observations, which have been overlooked by physicists dazzled by theories opposed to the natural conditions.—Further remarks on the question whether electricity is developed during the condensation of aqueous vapour, by Prof. Costantino Rovelli. In reply to the statements of Prof. Magrini, the author points out that, although his own experiments may have their weak side, the prolonged and repeated observations of Prof. Palmieri cannot be refuted by merely negative proofs.

SOCIETIES AND ACADEMIES

LONDON

Physical Society, February 12.—Annual General Meeting.—Prof. B. Stewart, F.R.S., President, in the chair.—In opening the proceedings the President regretted that in their Report the Council have to record the loss of one who took a prominent part in the proceedings of the Society, the late Dr. Guthrie. It was, however, satisfactory to learn that the appeal of the Guthrie Memorial Committee, under the presidency of Prof. Huxley, had been generously responded to. The Council also learn with regret from Dr. E. Atkinson that owing to pressure of work he is unable to retain the office of Treasurer to the Society, and desire to express their thanks to him for his past services. Prof. Rücker has consented to be nominated for the office thus rendered vacant, and the Council believe that by his election the connection between the Society and the Normal School of Science (which is so desirable) will be maintained.—The Report of the Council for the year 1886 was read and received, and the following gentlemen were elected Members of the Council for the present year:—President: Dr. Balfour Stewart, F.R.S.; Vice-Presidents: Dr. E. Atkinson, Prof. W. E. Ayrton, F.R.S., Shelford Bidwell, F.R.S., Prof. H. McLeod, F.R.S.; Secretaries: Prof. A. W. Reinold, F.R.S., Walter Baily; Treasurer: Prof. A. W. Rücker, F.R.S.; Demonstrator: C. V. Boys; other Members of Council: R. H. M. Bosanquet, W. H. Coffin, Conrad W. Cooke, Prof. G. Forbes, Prof. F. Fuller, Prof. J. Perry, F.R.S., W. N. Shaw, Prof. S. P. Thompson, C. M. Whipple, C. R. Alder Wright, F.R.S.—The President proposed the following resolution: "That at the end of Clause 11 of the By-laws, which says, 'Every candidate for admission into the Society shall be recommended by not less than three members, to two of whom he must be personally known,' there be added, 'When a candidate living abroad is a member of a recognised scientific Society, such membership may, subject to the approval of the Council, be held equivalent to the personal knowledge aforesaid.'" The resolution was carried, subject to confirmation by a special general meeting to be held on February 26.—A vote of thanks, proposed by Prof. Ayrton and seconded by Prof. McLeod, to the Lords of the Committee of Council on Education, for the use of the rooms and apparatus of the Normal School of Science, was passed unanimously.—The Hon. R. Abercromby proposed a vote of thanks to the officers of the past year for their gratuitous services, which was seconded by Prof. Pickering.—Sir Philip Magnus proposed a vote of thanks to the auditors, Colonel Festing and Prof. Fuller, which was seconded by Mr. Lecky, and passed unanimously.—Mr. J. Brown was elected a Member of the Society.—The following communication was then read:—Note on the tenacity of spun glass, by E. Gibson and R. E. Gregory. The authors have experimented on the tenacity of glass rods and fibres made from the same piece of glass. The fibres varied from 1/25 to 1/50 mm. and the rods from about ½ to 1 mm. in diameter. They find the tenacity per square centimetre of rods increases as the diameter decreases, as in ordinary wires, whereas with fibres this is not shown. Experiments were shown illustrating the method of working, and the highest tenacity recorded was for a fibre of ·0340 mm. diameter, which gave 466×10^7 dynes per square centimetre, a value about half as great as that for steel wires. The authors refer to Quincke's suggestion that the increased tenacity of small wires is due to surface-tension, and may be represented by $W = Ad + Bd^2$, where W is the breaking weight and d the diameter, but their own results with glass do not agree with this formula. Sir Philip Magnus asked if the diameters were measured at the point of rupture, if the elongation was determined, and whether the authors were able to suggest any other formula which would express their results. Mr. C. V. Boys remarked that the tenacity being so much affected by accidental circumstances, such as rate of cooling, no such formula could be expected. Prof. Rücker, referring to Quincke's experiments, said that the surface-tensions of metals calculated from them appear improbable. After some further remarks by the President, Prof. Ayrton, Mr. C. V. Boys, Prof. McLeod, and Mr. Gregory, the proceedings terminated.

Royal Meteorological Society, February 16.—Mr. W. Ellis, President, in the chair.—The adjourned discussion on the Hon. R. Abercromby's paper on the identity of cloud forms all over the world, and on the general principles by which their indications must be read, was resumed; and the following papers were read:—Remarks concerning the nomenclature of clouds for ordinary use, by Prof. H. H. Hildebrandsson; and Suggestions for an international nomenclature of clouds, by by the Hon. R. Abercromby. Both Prof. Hildebrandsson and Mr. Abercromby have paid great attention to the question of the forms of clouds, and having recently conferred together, they have agreed to recommend for international use the following ten principal varieties, viz.:—High-level clouds: cirrus, cirro-stratus, cirro-cumulus; middle-level: strato-cirrus, cumulo-cirrus; and low-level: cumulus, stratus, strato-cumulus, nimbus, cumulo-nimbus.—The influence of weather on the proportion of carbonic acid in the air of plains and mountains, by Dr. W. Marcet, F.R.S., and M. A. Landriset. The authors give an account of some experiments which they have made on the proportion of carbonic acid in the air at Geneva and on the summit of the "Dole," the highest point of the Jura chain, the difference in altitude being 4193 feet. The results of these experiments show: (1) that in fine clear weather on a mountain chain of moderate Alpine altitude, and in the adjoining valley or plain, the atmosphere holds the same mean proportion of carbonic acid at both places; and (2) that when the summit of a mountain chain is in a fog, a circumstance which frequently happens in an Alpine district, the air in the fog contains a smaller proportion of carbonic acid than it would hold in fine clear weather.—The Secretary, Dr. Tripe, read a letter received from Sir F. Abel, Organising Secretary to the proposed Imperial Institute, inviting the Society to draw the attention of the Fellows to the undertaking, with the view of their contributing towards it. The President stated that copies of the letter and of the accompanying paper, explanatory of the scheme, would be forwarded to each Fellow.

Mathematical Society, February 10.—Sir James Cockle, F.R.S., President, in the chair.—The following communications were made:—On the equation of Riccati, by the President (Prof. Hart, Vice-President, taking the chair).—The orthocentroidal circle (*i.e.* the circle whose diameter is the join of the orthocentre and centroid), by R. Tucker.—On polygons inscribed in a quadric and circumscribed about two confocal quadrics, by R. A. Roberts.—On the binomial equation $x^p - 1 = 0$; quinquisection, by Prof. Tanner.—Symmetrical determinant-formulæ in elliptic functions, by L. J. Rogers.—Notes on curves, by H. M. Taylor.—Some generalisations of differential formulæ connected with the change of the independent variable in a differential expression, with application to a new class of reciprocants, by C. Leudesdorf.

Geological Society, February 9.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—Evidence of glacial action in the Carboniferous and Hawkesbury series, New South Wales, by T. W. Edgworth David.—The terraces of Rotomahana, New Zealand, by Josiah Martin.—The eruption of Mount Tarawera, by Capt. F. W. Hutton. The paper began with a description of the country in which the eruption took place. From Tongariro to White Island, in the Bay of Plenty, a distance of 130 miles, there extends a belt, 20 or 30 miles wide, abounding in solfataras, geysers, hot springs, &c., and composed of volcanic rocks, chiefly rhyolite, with some augite-andesite. About the middle of this belt lie the mountain and lake of Tarawera, and two or three miles further south Lake Rotomahana, the spot where the famous Pink and White Terraces existed. Before the recent eruption there were no craters on Mount Tarawera, the form of which was a ridge, apparently due to denudation. Having described the eruption, Capt. Hutton briefly noticed the results of the eruption in the form of fissures on Mount Tarawera, the change of Rotomahana from a lake to a crater of larger dimensions, with precipitous walls, the formation of a new lake between this crater and Tarawera, and the formation of a number of small craters about Okaro. The materials ejected were composed of augite-andesite, and rhyolites, both compact and vesicular. The mineral structure and distribution over the surrounding country of various forms of pumice, scoria, and ash were described, and it was shown that there was a difference in the substances ejected from the mountain craters of Tarawera and those from the craters in the plain at Rotomahana and Okaro, the former comprising pumice and scoria, which were not thrown out from the latter, and but little steam issuing from the higher craters when compared with the enormous volumes emitted from the lower vents. The eruption was ascribed to the reheating of old lava-streams saturated with water. This reheating was apparently not due to crushing—for, had it been so, the preceding earthquakes would have been more violent—but probably to molten rock coming up from below and heating the rocks near the surface. The eruptions from Rotomahana and Okaro were purely hydrothermal.

Chemical Society, February 3.—Dr. Hugo Müller, F.R.S., President, in the chair.—The following papers were read:—The absorption of gases by carbon, by Charles J. Baker.—An explanation of the laws which govern substitution in the case of benzenoid compounds, by Henry E. Armstrong. Certain mono-derivatives of benzene, especially those containing a hydrocarbon radicle, one of the halogens, hydroxyl or amidogen, yield a mixture of the para- and ortho-di-derivatives in proportions which vary both according to the nature of the compound dealt with and of the reagent, and the conditions under which the change is effected; and if produced at all, the meta-derivative is formed in but a small proportion. If, however, the radicle present in the mono-derivative be—

$$NO_2, COH, CO_2H, SO_3H,$$

the meta-di-derivative appears invariably to be the chief product. Hitherto no explanation of this difference in the behaviour of the two series of mono-derivatives has even been suggested. In seeking to arrive at an explanation it is necessary to form a clear conception of the manner in which "substitution" is effected. The author is of opinion that in the first instance an additive compound is formed; and he points out that Kekulé has long since insisted in the plainest terms on this interpretation of those cases of change which are commonly spoken of as "double decompositions." He is inclined to believe that the tendency of negative to attract and combine with negative elements—to which he has of late frequently directed attention—is the effective cause; and that the additive compound is formed from those mono-derivatives which obey the "para-ortho law" by the fixation of the reacting molecule upon the carbon-atom which carries the radicle; separation of water or halogen hydride ensuing thereon, the radicle of the reacting molecule assumes the place either of an ortho- or of a para-hydrogen atom. It is easy to understand the formation of the ortho-di-derivative, as the hydrogen-atom displaced is associated with a carbon-atom contiguous to that to which the reacting molecule attaches itself. The formation of the para-compound is attributed by the author to the tendency towards symmetry, so frequently evidenced in cases of isomeric change and in other ways by benzenoid compounds; and not to the existence of any direct connection between carbon-atoms relatively in the para-position. The formation of meta-derivatives is believed by the author to result from the addition of the reacting molecule, not to the carbon-atom of the benzene-ring, but *to the radicle* which in the mono-derivative is attached to one of the carbon-atoms of the ring; he is, however, of opinion that in order to explain why the additive compound thus constituted yields a meta-di-derivative, it will be necessary to obtain further information regarding the "dynamics" of such changes.—Some derivatives of tetramethylene, by G. H. Calman and Dr. W. H. Perkin, Jun.—Derivatives of pentamethylene, by Dr. W. H. Perkin, Jun.—The decomposition of potassium chlorate and perchlorate by heat, by Dr. Percy F. Frankland and John Dingwall.—The action of chlorine on methyl thiocyanate, by Dr. J. William James.

PARIS

Academy of Sciences, February 14.—M. Gosselin, President, in the chair.—On waterspouts and M. Ch. Weyher's recent experiments, by M. Faye. While fully appreciating M. Weyher's novel and interesting essays, the author makes certain reservations, especially as regards the term *trombe marine* ("waterspout") applied by him to one of the results. This, he submits, was not a true waterspout, but only a rotatory movement of a volume of air without any defined limits, and with aspiration towards the axis of the ventilator. But a true waterspout is characterised by a cylindro-conical funnel sharply outlined, descending from the clouds to the ground or to the surface of the sea, without exercising on it any perceptible aspiration.—Note on MM. Paul and Prosper Henry's photograph of the nebula No. 1180 of Herschel's general catalogue, by M. Mouchez. During their photographic operations on Orion on January 27, MM. Henry obtained an image of a nebula of 3′ to 4′ diameter with stars of the 17th magnitude, invisible to the observer with the equatorial of the East Tower. This nebula, which has also since been photographed by Roberts in England, has now been identified with that discovered at the Cape by Herschel, and by him indicated with the number 1180 in his catalogue.—Reply to M. Houzeau's recent note on a method to determine the constant of aberration, by M. Lœwy. It is shown that M. Houzeau's method of determining the constant from the differences in right ascension or in declination as measured at different epochs, is liable to the most serious errors. In virtue of the diurnal movement, the two images are displaced in the field of the telescope at different rates of velocity and in any direction, their relative position changes from instant to instant, and under the given conditions cannot be accurately defined.—On a sandstone of organic origin discovered in the coal-fields of the Loire basin, by MM. Favarcq and Grand'Eury. Notwithstanding their chemical composition these remarkable deposits belong evidently to fresh-water organisms, which cannot at present be further identified. They abound especially in the Rive-de-Gier and Saint-Etienne districts.—The inauguration of railways in France: its true date, by M. Léon Aucoc. It is pointed out that the proposed celebration in 1887 of the fiftieth anniversary of this event rests on an historical error. The first line actually completed was that between Saint-Etienne and Andrezieux, 23 kilometres long, opened on October 1, 1828; that is, nine years before the assumed date 1837.—Remarks on the palæontological researches made in the Lower Tertiary deposits in the neighbourhood of Rheims, by M. V. Lemoine. The author gives the general results of his investigations carried on uninterruptedly for the last fifteen years, and constituting the Rheims district one of the points where the beginning of Tertiary life may be best studied in Europe. The fossil vertebrates alone studied by him now number 94, of which not more than 8 or 10 were previously known. Amongst them are 40 mammals belonging to 23 different genera, of which 8 only had hitherto been observed in later Tertiary beds.—On the mode of formation of the striated Bilobites, by M. Ed. Bureau. The author has obtained plaster casts of most of these Bilobites, from a careful study of which he concludes that they must represent imprints of animals on the sands of shallow Silurian waters.—Combined action of belladonna and opium in a case of acute diabetes, by M. Villemin. After the usual remedies had failed, this treatment was lately tried in an extreme case of diabetes at the Val-de-Grâce Hospital, with complete success.—Determination of the position of the shaft corresponding to a given position of the piston in a steam-engine, by M. H. Léauté. Two remarkably simple graphic constructions designed in 1869 by M. Marcel Deprez are described, by means of which the position of the shaft for each position of the piston may be determined with sufficient accuracy, when the length-ratio of connecting-rod and shaft is greater than 3.—On the application of photography to M. Lœwy's new methods of determining the elements of refraction and aberration, by M. Ch. Trépied. An inquiry is here made into the conditions and means by which M. Lœwy's new and effective method of photographic registration might be utilised in determining the elements of astronomic refraction.—Observations of Barnard's and Brooks's comets made with the 0·38 m. equatorial, Bordeaux Observatory, by MM. G. Rayet and Courty.—On surfaces where the difference of the chief radii of curvature is constant at each point, by M. R. Lipschitz.—On a certain class of recurrent sequences, by M. Maurice d'Ocagne.—On the specific heats of liquids, by M. Marcellin Langlois. By the process here described the author determines the specific heats of water, sulphuret of carbon, chloroform, chloride of carbon, ether, alcohol, and acetone.—Researches on the specific inducting power of liquids, by M. Negreano. The author determines the dielectric constants of a series of homologous and liquid carburets of hydrogen for the purpose of comparing the dielectric constants with the molecular weights and densities. He also determines the index of refraction of these different liquids with a view to the verification of Maxwell's law.—On the variable period of the current in an electro-magnetic system, by M. R. Arnoux.—Physical researches on the isomery of position, by M. Alb. Colson. Having succeeded in transforming orthoxylene and metaxylene into alcohols, glycols, ethers, &c., isomerous with the known compounds of paraxylene, the author here inquires whether bodies so closely related in their chemical properties may not also be connected by some physical relations. The best results have been obtained by the calorimetric process.—Action of the oxide of mercury on some dissolved chlorides, by M. G. André. In this preliminary paper the author deals with the chlorides of barium, calcium, strontium, and magnesium.—On the action of hydrochloric acid on the solubility of the chlorides, by M. R. Engel. His further studies enable the author to generalise the law already announced by him in the *Comptes rendus* for March 1886.—A new process of analysing the carbonic acid emitted, and the oxygen inhaled, in the act of breathing, by MM. M. Hanriot and Ch. Richet. The differential method here described as applicable, with some modifications, to the analysis of various gaseous mixtures, constitutes a simple and rigorous method for the quantitative analysis of those of respiration.—The formic salts, by MM. Gréhant and Quinquaud. The authors here discuss the question as to what becomes of the formic salts introduced into the system, and find that the formiate of soda injected into the digestive organs or into the blood mostly passes unchanged into the urine.—On the properties of colchicine, by MM. A. Mairet and Combemale. Their experiments on dogs and cats satisfy the authors that this substance is an irritant poison which attacks all the organs, but especially the digestive tube and the region of the kidneys.—On the effects of the transfusion of blood into the head of decapitated animals and men, by M. J. V. Laborde. The author refers to his numerous experiments on this subject, which were overlooked in the paper recently presented to the Academy by MM. Hayem and Barrier.—On the comparative morphology of the brain in insects and crustaceans, by M. H. Viallanes.—The males of *Lecanium hesperium* and the question of parthenogenesis, by M. R. Moniez.—On the zoological researches carried out during the second scientific expedition of the *Hirondelle*, in 1886, by Prince Albert of Monaco.

BERLIN

Physical Society, January 7.—Prof. von Helmholtz in the chair.—Dr. R. von Helmholtz developed theoretically the formulæ expressing the relations subsisting between vapour-pressure, the melting-point, pressure, and volume, and enabling the vapour-pressures in the fluid and solid state, or the freezing-points and the change of the melting-point with that of the pressure, to be calculated.—Dr. Thiesen, while engaged in working experiments instituted by Schellbach respecting the resistance of air, had found an expression for the force of resistance in accordance with which a medium with less interior friction must necessarily offer a greater resistance than did a medium with more friction. This induced him to carry out experiments of his own with cylindrical rods regarding the resistance of air. On a hardened steel point a brass cylinder open at the bottom and bearing at its lower end externally two conjoined pieces, into which the steel rods 1 metre long and 1 English inch thick could be inserted horizontally and diametrically opposite to one another, was able to rotate. By means of a cord circulation, the cylinder was set rotating, and the abatement of speed consequent on the resistance of the air was noted by each half-revolution being marked electrically. These experiments yielded the same formula for the resistance as had been obtained from the earlier experiments. Another important result was that the method employed for the measurement of the resistance of air had maintained its validity remarkably well. In the discussion following this address, Prof. von Helmholtz took part. He called attention to the formation of whirling surfaces and whirls on the rotating bodies, a matter which in a high degree complicated the phenomenon.—Dr. Thiesen made a further communication respecting the determination of the national standard kilogramme. The cylinders of platinum-iridium, weighing rather more than 1 kilogramme, which were cast in London, were tested in respect of their density, and so often as fissures were detected they were re-cast. They were thereafter polished and again tested. Forty-two such standards were next compared with one another, and their uniformity and non-liability to be affected by transport having been ascertained, they had then to be compared with the kilogramme of the Archives, and after examination by the International Commission were despatched to the different Governments.

BOOKS AND PAMPHLETS RECEIVED

Histoire des Sciences Mathématiques et Physiques, vol. x. : M. Marie (Gauthier-Villars, Paris).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. i. part 3 (Cunninghame, Sydney).—Nomenclature of Color for Naturalists : R. Ridgway (Little, Brown, and Co.).—Bulletin of the Philosophical Society of Washington, vol. ix. (Washington).—Hourly Readings, 1884, part 2, April to June (Eyre and Spottiswoode).—The Origin of the Fittest : E. D. Cope (Macmillan).—Geographical and Geological Distribution of Animals : A. Heilprin (K. Paul).—Hints for the Solution of Problems of Solid Geometry : P. Frost (Macmillan).—Levelling and its General Application : T. Holloway (Spon).—Observations Météorologiques de Godthaab : A. F. W. Paulsen (Gad, Copenhague).—*Challenger* Expedition Reports—Zoology, vol. xvii. ; Botany, vol. ii.—Philosophische Studien, Vierter Band, 1 Heft (Engelmann, Leipzig).—Encyklopædie der Naturwissenschaften, Erste Abth., 50 Lief. ; Zweite Abth. 39, 40, 41 Lief. (Trewendt, Breslau).—City Government of Boston, Mass. : J. M. Bugbee (Baltimore).—Annual Address to the Asiatic Society, Calcutta, February 2, 1887 : E. T. Atkinson.—Halifax, Annual Report of Public Library Committee, 1885-86.—Beiblätter zu den Annalen der Physik und Chemie, Band x. (Barth, Leipzig).

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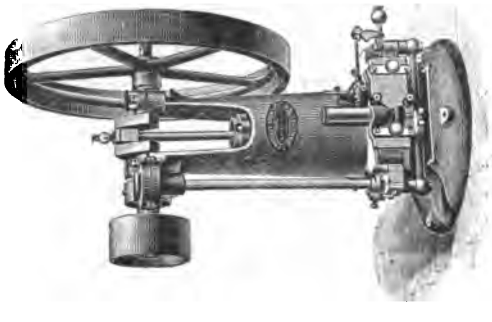

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So, too, the definition of the "volt" would have been clearer if the excellent illustration of difference of potential given in § 40, Fig. 28, had been carried a little further, and it had been pointed out that, just as the water loses potential energy in falling from one level to another, and that loss is measured by the quantity of water multiplied by the difference in pressure, so the electric current loses energy in passing from one point to another, and that loss is measured by the quantity of electricity multiplied by the difference of electrical potential. But these, perhaps, are points which can be better brought out by a teacher in explaining difficulties to his class.

The book is, we believe, the first in England which accepts distinctly the resolutions of the Paris Congress as a basis. Another novelty to be found in it is the use of the letters P.D. (potential difference) for the old abbreviation E.M.F. This is a change for which, if it can be satisfactorily introduced and accepted, all teachers will be thankful, for it will get rid of the confusion existing between the resultant electrical force at a point, and the electromotive force between two points, which is not a force at all.

The plan of the book has been already indicated. Starting from the definition of an ampere, the various means of measuring currents are described, and full details are given of the methods for carrying out the experiments, for recording results graphically and otherwise, and for drawing conclusions from the experiments. The illustrations show clearly the arrangement of apparatus in each case. In Prof. Ayrton's laboratory the apparatus required for any one series of observations is mounted permanently on the same board; the student finds everything ready, and the necessary connexions made when he begins. As to the desirability of this, there will no doubt be some difference of opinion; but with large classes of beginners some such plan is necessary. Thus, Fig. 15 shows the apparatus for investigating the action of a current on a magnet. The large coil C C is so arranged that the current can be made to traverse it eight, twelve, or sixteen times, and its effect on the magnet *g g* observed; or the coil C C can be replaced by *c c* of half the radius, which has four turns on it; or, again, C C can be used simultaneously with *c c*, the current being

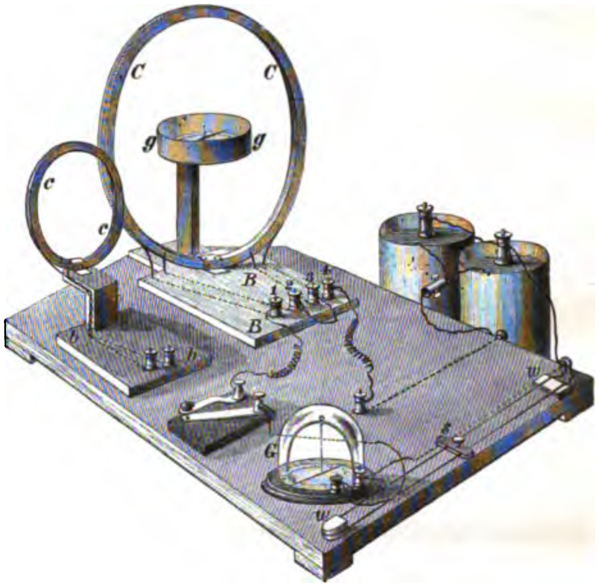

Fig. 15.[1]

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[1] We are indebted to Messrs. Cassell for the blocks used to illustrate this article.

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sent in opposite directions through the two. *w w* is a resistance by means of which the current may be varied at will, and G a galvanometer.

Chapter III. introduces us to difference of potential, and here the writer describes in a practical form the experiments for the verification of some of the ordinary laws of statical electricity.

Ohm's law and its proof follow in Chapter IV., and it is shown that the resistance of a conductor remains unchanged so long as its other physical conditions are the same. § 80 gives the definition of the "legal ohm." It is not quite accurate to speak of its having been legalised, at any rate in this country; though, as the letter from the B.A. Committee on Electrical Standards, which is printed at the end of the preface, shows, the question of its adoption as a legal standard is now before the Government. Various methods of comparing resistances follow, with full practical details, but the important one due to Prof. G. C. Foster, for comparing two nearly equal resistances, has been omitted from § 97.

Batteries and other forms of current-generators are next considered, with details as to their construction and use; and an account of the methods of determining thei E.M.F. and resistance is given.

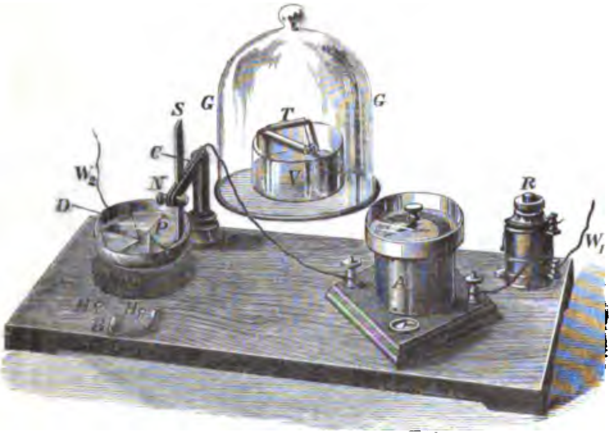

FIG. 157.

"Insulation" is the title of Chapter VI., and many o the practical hints given under this head are of grea value.

In the next chapter, condensers, and the methods of constructing them and of measuring their capacities, are treated of. We find also a chapter devoted to commercial ammeters and voltmeters, which gives in an easily accessible form particulars as to some of the best known of these instruments, with practical methods of testing and calibrating. In Fig. 157 we have the arrangement of apparatus for comparing an ammeter with a silver voltameter. R is an adjustable resistance of a useful character, made of a number of washers of carbonised cloth which can be pressed into close contact by a screw. Latimer Clark's cell is described in § 214, in the H form. A simpler pattern consists of a test-tube with a platinum wire sealed through the bottom. The end of this is covered with pure mercury, and above this is a layer of mercurous sulphate, with a saturated solution of sulphate of zinc above all. A rod of clean zinc dips into the zinc sulphate passing through the cork which closes the test-tube, and the whole is kept tight with marine glue.

In an appendix Kirchhoff's laws of divided circuits are considered. The method of treating such problems introduced by Maxwell, and which has recently been illustrated at some length by Prof. Fleming, is also referred to.

Another point of importance which calls for notice is the number of examples introduced by way of illustration; some of these are completely worked out, others left as exercises for the student.

The book concludes with some specimens of the instructions for experiments which are given to students at the City and Guilds Institution, with the apparatus with which each experiment is performed. Of these instructions there are four; and many who use the book will wish their number was larger. We will close this notice by quoting the last of them.

To calibrate an ammeter by means of a silver voltameter (the apparatus required has been figured above): —

"PRELIMINARY.—The voltameter consists of a platinum dish containing a 25 per cent. solution of silver nitrate, and in which a silver plate is immersed. An adjustable carbon resistance is provided, by means of which the current passing through the voltameter can be maintained constant during each experiment, and can be varied in the different experiments.

"EXPERIMENTS.—(1) Carefully clean, dry, and weigh the platinum dish, the *approximate* weight of which is 78 grammes.

"(2) Pour the solution of silver nitrate into the dish, and place it on the three brass pins provided for its reception, and which are electrically connected with the left-hand binding-screw on the board. Immerse the silver plate in the solution, and clamp it in such a position that its edges are equally distant from the sides and bottom of the dish.

"(3) Turn the small milled head at the top of the ammeter so that the pointer of the ammeter comes opposite the zero on the scale, if not there already. Place the copper connecting-wire in the mercury cups marked A and C (which cuts out the voltameter), and adjust the carbon resistance until a convenient current flows round the ammeter. Remove the connecting-wire.

"(4) Quickly insert the connecting-wire in the mercury cups marked A and B, carefully noting the instant at which the circuit was completed. Allow the current to pass for a convenient time (ten to thirty minutes, according to the strength of the current used), and keep the current constant by the adjustable resistance. Note the temperature of the room during the experiment, and, at the end of the interval decided on, quickly break the circuit.

"(5) Empty the solution from the dish into its bottle, and carefully wash the deposited silver with distilled water. Then fill the dish with distilled water, and allow it to stand ten to fifteen minutes Again wash with water, alcohol, and ether, dry over the spirit-lamp, and cool in the desiccator.

"(6) Carefully determine the increase of weight due to the silver deposited on the dish.

"(7) Calculate the strength of current used in the experiment, assuming that one ampere deposits 1·11815 milligrammes of silver per second.

"(8) Repeat the experiment with several different strengths of current.

"(9) Tabulate your results in some convenient form, and write them with your name on the card, on which you will find recorded the results of previous experiments."

SPOLIA ATLANTICA

Spolia Atlantica. (1) Contributions to the Knowledge of the Salpidæ, by M. P. A. Traustedt; (2) Remarks on some of the Oceanic Annulata, by G. M. R. Levinsen; (3) Contributions towards the Morphology and Systematic Arrangement of the Pteropoda, by J. E. V. Boas. (Copenhagen, 1885-86.)

THE three monographs which at the instance, and under the supervision, of the Directors of the Zoological Museum of Copenhagen, have been included in one quarto volume under the title of "Spolia Atlantica," originally appeared in the Transactions of the Danish Royal Society of Natural Sciences. But although bound together, each monograph in this *édition de luxe* is complete in itself, with separate title-page, table of contents, descriptive plates, and all other necessary means of separate reference, while the convenience of readers not acquainted with Danish has been amply considered by the addition of Latin and French translations of the descriptions of the animals, and of many other important parts of the text.

The first of this triplet of monographs, which deals with the so-called "aggregate" and "solitary" forms of the several species of Salpæ, is based on a study of the exceptionally complete collections preserved in the Zoological Museum of Copenhagen, for which that institution is mainly indebted to Prof. Steenstrup, at whose suggestion and under whose direction Herr Traustedt compiled his memoir. The monograph presents a clear and comprehensive description of all the well-established species of Salpæ with their distinctive dual forms, and unqualified praise may be given to the care with which the figures have been drawn, and the admirable manner in which, by means of pale blue outlines, the delicacy and transparency of the bodies of the animals have been represented.

In treating of the Salpidæ, it is impossible to forget how much of our knowledge of these curious animals is due to the observations of Chamisso, the clever author of "Peter Schlemihl," who, while serving as naturalist in the exploring expedition of the Russian commander Kotzebue, first discovered that the "aggregate" or chain Salpa and the "solitary" Salpa were not distinct species, as had been supposed, but only parts of the perfect organism of one species. By the discovery of this fact, which Chamisso ingeniously, but, as subsequent investigations have shown, too fancifully, explained on the hypothesis that these animals were subject to a law of "alternation of generations," new and highly important paths of morphological inquiry were opened. Yet, singularly enough, nearly thirty years passed after the publication, in 1819, of Chamisso's treatise "De Animalibus quibusdam e Classe Vermium Linneana (de Salpis)" before his observations were tested by further scientific investigation. About the middle of the century Meyen and Vogt turned their attention to the curious and interesting phenomena connected with the embryonic development of the Salpæ. These inquiries were soon followed by the still more important researches of Profs. Krohn and Huxley, the latter of whom in a paper entitled "Observations upon the Anatomy and Physiology of Salpa and Pyrosoma," which appeared in the Phil. Trans. 1851, has shown that Chamisso erred in his explanation of the nature of a "chain Salpa," which, to use Mr. Huxley's words, "is nothing more homologically than a highly individualised generative organ."

Herr Traustedt does not enter into the question of the embryonic development of the Salpidæ, and hence his work gives no information regarding the physiology of these animals, nor does he in any way refer to the various hypotheses that have been advanced in explanation of the character of the "aggregate" and "solitary" forms. For such information the student must go elsewhere. As a guide to the anatomical structure of both forms in the eleven species described and drawn by the author, the memoir will, however, be found of great service, while it contains much useful information as to the geographical distribution of these animals not to be found elsewhere.

In the treatise on "Some Oceanic Annulata" Herr Levinsen supplies many interesting details regarding various members of the families Alciopidæ and Typhloscolecidæ, together with descriptions of several species of Sagitta, to which are added lists of their geographical distribution. In this, as in the memoir on the Pteropoda, the illustrations are worthy of all praise.

The memoir by Dr. Boas, which constitutes the last and longest of the series, treats at great length of the morphology, systematic arrangement, and geographical distribution of the Pteropoda. The materials employed by the author were derived in part from the extensive collections in the Zoological Museum of Copenhagen made by, or under the direction of, Prof. Steenstrup, and in part from numerous specimens placed at the writer's disposition by Profs. Dohrn, Möbius, Leche, and Spengel. It is worthy of note that a very large proportion of the specimens referred to as belonging to the Museum of Copenhagen were obtained from amateur collectors; Danish naval officers, captains of merchant ships, and travellers having responded with alacrity to Prof. Steenstrup's appeal for help in obtaining samples of these and other animals from remote regions.

OUR BOOK SHELF

Complete Hand-book on the Management of Accumulators. By Sir David Salomons, Bart. Second Edition, revised and enlarged. (London: Whittaker and Co., 1887.)

THE author has for some years past had an installation at his country residence for the purpose of lighting it and for working motors which drive the machinery in his large and well-equipped workshop. He has used accumulators, as he informs us, ever since they may be said to have been produced in commercial form in 1882. No expense nor trouble have been spared in making this installation a model one, and experiments have been made on many points in connexion with the subject. As the whole installation has been made and worked under his immediate personal superintendence, he has acquired a considerable amount of experience, the result of which, as far as it regards the management of accumulators, he places before the public in this work.

After a general description of cells of the E.P.S. and Elwell-Parker type, he proceeds to give directions for setting up and charging them. The causes of, and remedies for, "buckling" and "scaling" are discussed. The harm caused by too rapid a discharge is pointed out, and methods for preventing it are explained, as well as various devices for regulating the E.M.F. of the charging current and that on the line. The method of cleaning and "re-pasting" the plates is explained, and various hints are given for the management of the battery. In an appendix is a description of the arrangement of his accumulator house, a photograph of the interior of which forms the frontispiece to his book. He also gives a brief account of the history of the installation, from which we learn that the total cost of buildings and installation has been about 6000*l.*, that the number of lamps is about 500 of 20-candle power, but that the greatest number used at any one time has rarely exceeded 200, and that an arc lamp taking 40 or 50 amperes and one or two motors have been used together with them. The expenses—including wages, coal, oil, waste, washers, repairs, lamp renewals, &c.—were, in 1886, 210*l.*, or at the rate of ½*d.* per 20-candle lamp per hour. He does not, however, say anything as to the cost of the accumulators; and as to the length of time which they may be expected to last he only says vaguely that "the cells may last for years in perfect order if all the instructions here given are properly attended to." His present accumulators were only put up in August 1885, those previously erected having proved to be unsatisfactory.

It is a pity that the useful information in this work is not conveyed in better language. In what is intended as a workshop hand-book we do not look for the elegance of an Addison; but we do want plain English. As an example of the language we may quote the following sentence from the preface: "Only cells of the Electrical Power Storage Company, of Messrs. Elwell-Parker, and their type, are dealt with, because at this moment there is probably no other better kind, or largely in use." The meaning of this is clear, though even that is more than may be said of other sentences in the book.

Among minor faults we protest against the coinage of such a hideous word as "acidometer," by which, apparently, is meant the instrument commonly known as a hydrometer; we do not think "s.g." is an improvement on the ordinarily accepted abbreviation for specific gravity; and we object to the plural "dynamos" as against the ordinary rule of our language which gives us "potatoes" and "echoes."

A more serious error is found in the "Index of Terms" on the back of the title-page—

"Watt = volt × ampere = measure of force or energy."

We should recommend the author to get the work revised by some onewho understands the rudiments of science as well as those of the English language.

School Hygiene: The Laws of Health in Relation to School Life. By Arthur Newsholme, M.D. (London: Swan Sonnenschein, Lowrey, & Co., 1887.)

About the importance of the matters dealt with in this little book there can, of course, be no dispute; and, as Dr. Newsholme points out in his preface, they have engaged the serious attention of many School Boards and Committees, and been made the subject of a good deal of useful legislation. Most school-managers still have something to learn about the principles of school hygiene, and many of them will, no doubt, find in Dr. Newsholme's volume exactly the kind of information they want. He discusses the subject under two heads, "Schools" and "Scholars." Under the first head he presents his ideas on questions connected with the choice of sites for schools, the construction of school buildings, school furniture, lighting of school-rooms, general principles of ventilation, natural ventilation, ventilation and warming, and draining arrangements. In the part relating to "Scholars" he has chapters on mental exercise, excessive mental exercise, age and sex in relation to school work, muscular exercise and recreation, rest and sleep, children's diet, children's dress, baths and bathing, eyesight in relation to school life, communicable diseases in schools, and school accidents. Dr. Newsholme has studied his subject thoroughly, and his conclusions are all the more valuable because they have been to a large extent suggested by his experience as a medical officer of health, and as a medical referee for various schools and training colleges.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Mass, Weight, and Dynamical Units

If the laws of dynamics were made solely for the use of engineers, as a celebrated engineer declared of rivers that they were intended to feed canals, it might be conceded to Prof. Greenhill that it would not be *necessary* that the more abstract notion of mass should be distinguished from that of weight, and that the fundamental equation of dynamics might conveniently be taken as $P = W\frac{f}{g}$, where W is the weight (or mass) of the body moved expressed in pounds or tons, &c., and P the force producing in it the acceleration f, and reckoned in the same units. But a pound weight as a force is a variable unit, unless it is taken at some particular place (as Greenwich), and then the corresponding value of g must be employed (though the variation of g on the surface of the earth is not so great as often to need to be taken account of by the engineer); and so the astronomer and physicist, as well as the student of abstract dynamics, are right in demanding a less arbitrary measure of force than one founded on the force acting vertically downwards on a body at the earth's surface, as well as an absolute constant belonging to each body (its *mass*) independent of time and place. I conceive, therefore, in spite of Prof. Greenhill's arguments, that, in the interest of clearness of thought, as well as to avoid the practical inconvenience of a variable unit of force, the notions of mass and weight must be kept distinct, and the equation $W = Mg$, as a special case of the general equation $P = Mf$, still insisted on by all teachers of dynamics, at any rate to non-engineering students; while it would be well for engineers also (*me judice*) to have their dynamics cast in the same mould as the rest of the scientific world.

I should not have troubled you with the above very obvious remarks had I not wished to observe, *à propos* of Mr. Geoghegan's suggestion of names for units of velocity and acceleration in Nature of April 7 (p. 534), that it is highly desirable that a convenient and consistent *notation*, as well as *nomenclature*, should be adopted for the several dynamical units.

With respect to the particular suggestion that *vel* and *cel* (or *velo* and *celo*, as used by Mr. Lock in his forthcoming book, "Dynamics for Beginners") should be employed for the units of speed and acceleration in the foot-second system, I regard it as an almost fatal objection that the accepted C.G.S. system having appropriated the words *dyne* and *erg* for its units of force and work has a prior right to *vel* and *cel* for its units of speed and acceleration; namely, the centimetre per second, and the centimetre per second per second. If so, the units of the foot-second system might be called the *footvel* and the *footcel*.

Names for the dynamical units are, however, I think, of less importance than a convenient and suggestive notation for them. I have endeavoured to devise such a notation (or, rather, to fill up and complete notations which have more or less come into use) in a table, which is now under the consideration of a Committee of the Association for the Improvement of Geometrical Teaching, and which is substantially as follows.

In a perfectly general system, let **L, M, T** stand respectively for the fundamental units of length, mass, and time, and let **V, A, U, Δ, E** stand for the units thence derived of speed, quickening (acceleration), momentum or impulse, force, and energy or work respectively: then the statement that the unit of speed is the speed of the unit of length *per* unit of time is naturally expressed thus, $V = L/T$. On the same principle we should have—

$$A = V/T = L/TT,\ U = MV = ML/T,\ \Delta = MA = ML/TT,$$
$$E = L\Delta = LML/TT.$$

This may be simplified, using the analogy of the fluxional notation, by writing $\dot{L}$ for L/T and L for $\ddot{L}/TT$. Then $\dot{L}, \ddot{L}$ may take the place of V and A, and we have—

$$U = M\dot{L},\ \Delta = M\ddot{L},\ E = L\Delta = LM\ddot{L}.$$

Names for the several units are hardly needed in the general system.

In the C.G.S. system the units of length, mass, and time are respectively the centimetre, gramme, and second, denoted by C, G, S. Then if U_G, Δ_G, E_G denote the units of momentum or impulse, force, and energy or work, we have, on the same principle as before, $\dot{C}, \ddot{C}$ to denote the speed of a centimetre per second and the acceleration of a centimetre per second per second respectively, and the equations—

$$U_G = G\dot{C},\ \Delta_G = G\ddot{C},\ E_G = C\Delta_G = CG\ddot{C}.$$

If *names* for all these units are required, we may use these: *vel, cel, mom, dyne, erg;* and we may say, a *mom* is a *gramme-vel*, a *dyne* is a *gramme-cel*, and an *erg* a *centimetre-dyne.*

In the F.P.S., or British system, the units of length, mass, and time are respectively the foot, pound, and second, denoted by F, P, S. Then, if U_P, Δ_P, E_P denote the units of momentum or impulse, force, and energy respectively, we have, on the same principles, $\dot{F}, \ddot{F}$ to denote the speed of a foot per second and the acceleration of a foot per second per second respectively, and the equations—

$$U_P = P\dot{F},\ \Delta_P = P\ddot{F},\ E_P = F\Delta_P = FP\ddot{F}.$$

I propose as names for these units: *footvel*, *footcel*, *poundem*, *poundal*, *pounderg*. I should have called the unit of force a *pound-dyne* or *poundyn*, but that *poundal* has already obtained general acceptance.

The foregoing are all absolute units. The corresponding (Greenwich) gravitation-units are the *pound-weight* = 32·19 *poundals*, the *foot-pound* = 32·19 *poundergs*, and the *second-pound* (as it has been proposed by Prof. Unwin to name the gravitation-unit of momentum, or time-integral of a pound-weight through one second) = 32·19 *poundems;* so that to convert absolute F.P.S. units into gravitation-units, or *vice versâ*, it is only necessary to divide or multiply by 32·19, since the acceleration due to gravity at Greenwich = 32·19 $\ddot{F}$.

It is, I think, comparatively unimportant whether the *names* above suggested are, all of them, accepted or not; but the *notation* will, I believe, be found a great aid to the beginner in fixing in his mind the dimensions of the different magnitudes, and an effective safeguard against the too common confusion of units of force, impulse, and work. I doubt whether in speaking much would be gained by saying "*footvel*" instead of "foot per second," or "*footcel*" instead of "foot per second per second," while in *writing*, the symbols $\dot{F}$ and $\ddot{F}$ might be always used and read either way.

Robt. B. Hayward

Harrow, April 12

Units of Weight, Mass, and Force

Far be it from me to interfere between Mr. Alfred Lodge and Prof. Greenhill; but, whilst leaving Mr. Lodge to his fate and Prof. Greenhill, perhaps, as an engineer, I may be permitted to offer a few remarks on the general question. First, then, it appears to me that Prof. Tait's thoroughgoing condemnation of certain phrases of the engineering vernacular, and of the grave errors of certain writers, has been strained in some quarters to mean a general charge against engineers of inability to think or write clearly on the physical laws which lie at the root of their every-day practice. Such an unqualified charge is on the face of it absurd; for otherwise we must confess that the lives' work of Thomson and Tait has been a total failure as regards its influence on the truly practical men of their generation. Can this be so?

Prof. Greenhill's remarks on the abominable semi-numerical equation $W = Mg$, or $\frac{W}{g} = M$, I most heartily welcome [where W is a mass and g a numeric; the moment writers on dynamics, who use the gravitation system, pass from merely proportional equations to their physical interpretation, then must we face with them this most wretched equation]. But perhaps it will be at once a surprise and a gratification to Prof. Greenhill to know that a whole (academic) generation of Scottish University engineering students has been taught to eschew this same equation as an unclean thing, and to adopt the mode of thought clearly set forth in the last two paragraphs of his letter of February 28. Some five or six years ago I was myself so taught by one who is now, alas! no more. The possibility of thus clearing of cant not only the engineering but the purely mathematical mind seems to me, as indeed Prof. Greenhill indicates, to be a direct consequence of the acceptance by Thomson and Tait of the British Imperial pound as the unit of mass or quantity of matter.

Having fixed, then, for good and all, the unit of mass, and taking the British foot and the second as the units of time and length respectively, the unit of force defines itself in virtue of Newton's Second Law. To this unit—the British unit of force—Prof. James Thomson has given, as nearly everyone knows, the name "poundal." Now the most convenient practical unit of force, for physicists as well as engineers, is not the poundal, but the gravitation at the earth's surface of the unit of mass, a quantity which is not absolutely constant, the inconvenience so arising being, however, practically unimportant, or at most involving a reduction to an arbitrary standard. To pass, then, from a force expressed in poundals to a force expressed in units of gravitation of the Imperial pound at the standard place, one simply wants to know how many poundals go to the gravitation of the Imperial pound at that place—in other words, the change ratio. The answer is simple: the numeric g for the standard place; for a force equal to the gravitation of the Imperial pound at the standard place acting upon a mass of one Imperial pound would generate a momentum per second of g (numeric) pounds mass × feet per sec. per sec.; and the poundal a momentum per second of one pound mass × feet per sec. per sec.; and by Newton's Second Law the ratio of the forces is, therefore, the numeric g. What more does the physicist or engineer want to know? How many poundals go to the gravitation of a ton mass at the standard place? Answer, the numeric 2240 g. Could anything be simpler? The difficulty is to find the difficulty, or to assign the *raison d'être* of the so-called gravitation unit of mass.

When the old unnatural gravitation unit of mass is abandoned, and the transition from the natural unit to the gravitation unit of force made by means of the change ratio, the vicious use of the word mass to denote the result of dividing by the numeric g the mass of a body in standard pounds (viciously on the same system called the weight and denoted by W) ceases; and the dire confusion between weight and mass becomes a thing of the past. The emancipation of the term weight from its bondage to mass would appear to have afforded opportunity for its use wherever it might be of service in suggesting or denoting gravitation. For example, there are three units, each called a pound, viz. the Imperial British unit of mass, the gravitation of the same mass at any the same place on the earth's surface, and the pound sterling. Physicists, like Sir Wm. Thomson and Prof. Tait, use terms such as "pound weight," "gramme weight"; similarly we have "pound mass" coming into use; and probably we shall soon hear of "pound money." Prof. Greenhill, with his strong engineering sympathies, objects to the time-honoured "pounds per square inch" being rendered "pounds weight per square inch"; and if I may presume to offer an opinion, the old phrase is already cumbrous enough. Still, if Prof. Tait and Prof. Greenhill ultimately agree that there is anything to be gained in perspicuity, for the sake of the weaker members perhaps it might be well for us to put in practice, on some occasions, the injunction to sacrifice all rather than cause our brother to offend.

Some one might possibly step in to draw attention to the fact that the pages of even our own great high priest of exact applied science are disfigured by $\frac{W}{g}$; but sure I am that were Rankine now with us he would lead the way with Prof. Greenhill in a crusade against the apologists for the obscurity of which W/g is the symbol. Take, for example, Rankine's bold introduction of the dynamical unit of quantity of heat. Take the opinion of

Clausius, than whom none could be a more unprejudiced witness. "At the same time in the theoretical development of the mechanical theory of heat, in which the relation between heat and work often occurs, the method of expressing heat in mechanical units effects such important simplifications that the author has felt himself bound to drop his former objections to the method on the occasion of the present more connected exposition of that theory." ARCHD. C. ELLIOTT

Edinburgh, April 18

Seismometers

I HAVE long ago learnt not to look for any fair recognition of my work in seismometry on the part of Prof. John Milne, and when he accuses me of appropriating without acknowledgment the work of others it is time to decline further controversy with him. The points raised in his last communication (NATURE, April 14, p. 559) are sufficiently answered in mine of December 11 (p. 172). I there quoted part of a letter written by Prof. Chaplin, now of Harvard University, then of Tokio, and Secretary of the Seismological Society of Japan, under whose eyes the events occurred to which Prof. Milne refers. I did not quote the whole of Prof. Chaplin's letter, because it contained sentences I was unwilling to give except under the strongest provocation. After referring to my seismograph in the words already quoted (p. 172), Prof. Chaplin continues :—

"I do not remember that in the discussions on your machine Mr. Gray ever claimed to have invented a similar machine, and I am surprised to know that he makes that claim now. On this and other points it appears to me that Messrs. Gray and Milne have not treated your inventions and investigations with fairness, and that you have just grounds for complaint. I am willing you should make such use of this note as you see fit."

As to the question of priority, this judgment, from a man at once unprejudiced, most competent to form an opinion, and fully informed of the matter in dispute, must (so far as I am concerned) close a discussion of which your readers cannot but be weary. With your permission I shall give, in a later number of NATURE, an example of the excellent work which Prof. Sekiya is now doing with my instruments in Japan.

J. A. EWING

University College, Dundee, April 16

April Meteors

THE Lyrids have, this year, offered a somewhat scanty display, though a few brilliant meteors have been seen shooting from the usual radiant-point.

In 1884 April 19, this shower was very rich, the horary number of its meteors for one observer being about 22, but in the following year, 1885, it exhibited a considerable decline, the hourly rate being only 3. In 1886 I obtained no observations, owing to the bright moonlight and in the present year, on April 20, the horary number was slightly more than 2, so that the numerical character of the recent display has fallen far short of some of its apparitions in preceding years.

On the night of April 17, this year, the shower had not visibly opened, for none of its meteors were recorded in a 2½ hours' watch. On each of the nights of the 18th and 19th the sky was closely observed for 4½ hours, but the Lyrid shower was very feeble, and only furnished 1 meteor per hour. On the 20th, in 3 hours I noted 7 Lyrids, and these were brilliant.

The average radiant-point from the three nights was at 269° + 32°, and there is confirmation that this point advances in R.A. with the time, though not to the marked degree ascribed in NATURE for May 7, 1885, p. 5. But the meteors from this stream have been so scarce at their late recurrence that it has been very difficult to ascertain the exact radiant for each night. Moreover, these Lyrids move with great apparent velocity, flashing out with extreme suddenness and they are gone, together with the faint streaks sometimes accompanying them, before the eye is enabled to catch the directions with satisfactory precision.

On the four nights April 17 to 20 inclusive, I noticed 70 shooting-stars belonging to the minor systems of the Lyrid meteoric epoch, and amongst these the best was that of a radiant of very swift, short meteors at 231° + 17°, a few degrees west of β Serpentis. This stream is not new, for I saw a well-defined shower of Serpentids from the same point during my observations of the Lyrids in 1885, on April 19–20 (NATURE, May 7, 1885, p. 6).

In this and in previous years I have also recorded some meteors ascending in very long flights from a radiant centre close to θ Libræ, at 235° – 15°. This is the only observation of this shower at the April period, though Lieut.-Colonel Tupman found a pair of radiants near the position assigned in the first week of March 1869–70.

I subjoin a short list of bright meteors seen here while watching the progress of the Lyrids, and I should be glad to hear that any of these had been observed elsewhere.

Date	Hour	Mag.	Apparent Path		Notes	Radiant
			From	To		
1887	h. m.		° °	° °		
April 19	13 13	♀	269 + 11	269 + 1	Swift, streak	Lyrid
19	13 46	♃	308 + 61½	56 + 65½	Swift, streak	279° + 13°
20	9 48	1	211 + 7	194 – 4	Very swift	Lyrid
20	10 29½	>1	243 + 14	234 + 5	Swift, streak	Lyrid
20	10 47	1	239 + 53	269 + 62½	Slow, train	206° + 18°
20	12 28	>1	308 + 40	316 + 40½	Very swift	Lyrid

Bristol, April 22 W. F. DENNING

Vertical Decrement of Temperature and Pressure

IN NATURE of March 10 (p. 437), Mr. Maxwell Hall gives an interesting table of the vertical distribution of temperature and pressure in Jamaica, and, apparently in happy ignorance of the dangers of the process known as extrapolation, goes on to apply the results of observations extending to a maximum height of only 7400 feet to the determination of the probable temperature of meteorites in extra-terrestrial space. As he expresses a desire to know whether any similar results have been found in India, and as I have on several occasions during the past ten years discussed the vertical distribution of temperature and pressure in this country, I gladly take this opportunity of referring him to my papers on the meteorology of the North-West Himalaya, and on the temperature of North-Western India, published in the "Indian Meteorological Memoirs," vols. i. and ii. From the latter I extract the following table on the mean decrement of temperature up to a height of 12,000 feet, computed from the observations of twenty-five stations combined in various ways. For each month an interpolation formula of the form

$$T = T_0 + ah + bh^2 + ch^3,$$

was computed, and by its means the decrements from sea-level to 1000 feet, 1000 to 2000 feet, &c., were calculated. Finally, the average decrement for the twelve months was computed, and is here given in an abridged form. The curves for the several months differ very widely from one another, those for the summer giving the most rapid decrement at sea-level, and the decrement increasing with altitude in winter :—

Height Feet	Mean temperature decrement ° F.
0 to 2000	6·16
2000 to 4000	5·87
4000 to 6000	5·61
6000 to 8000	5·37
8000 to 10000	5·16
10000 to 12000	4·98

The mean height of the barometer at sea-level in the region in question, the centre of which lies a little north of Simla, is about 29·8 inches ; the mean at 6000 feet is 24·1 inches, and the mean at 12,000 feet about 19·4 inches. With these data, and adopting Mr. Hall's formula

$$\delta T = \lambda.\delta P + \mu.(\delta P)^2,$$

we find λ = 2°·979 and μ = 0°·02. These coefficients do not differ widely from Mr. Hall's values, which are 2°·92 and 0°·08 respectively. At the limit of the atmosphere, where δP = 29"·8, δT would be – 106°·5, which would give, as the mean temperature of external space, about – 30° F., the mean temperature at sea-level being 77° F.

Taking the simplest formula, $\delta T = \lambda.\delta P$, we find λ = 3°·19, which is almost identical with the value quoted by Mr. Hall from an early volume of NATURE, but which, if it held good to the limit of the atmosphere, would make the temperature of external space about – 18° F., since the mean temperature at sea-level is 77° F.

The only conclusion to be drawn from such observations is that the vertical decrement of temperature on mountains varies greatly with locality as well as season, and the results obtained for one locality cannot be fully applied to another, much less extended to determine the temperature at the superior limit of the atmosphere.

An interesting point of resemblance between Mr. Hall's observations and those made on the Himalaya is that the diurnal range of temperature diminishes to a minimum at about 5000 feet, and then increases with increasing elevation.

Allahabad, March 30 S. A. HILL

Royal Society's *Soirée*

MAY I be permitted, through the columns of NATURE, to ask, on behalf of the Sub-Committee appointed to make arrangements for the forthcoming *soirée* of the Royal Society, that Fellows and others who have apparatus or objects of scientific interest suitable for exhibition on that occasion will communicate at once with the Secretaries or myself. HERBERT RIX,

Royal Society, Burlington House Asst. Sec. R.S.

HOMERIC ASTRONOMY[1]

II.

TURNING to the second great constellation mentioned in both Homeric epics, we again meet traces of remote and unconscious tradition. Yet less remote, probably, than that concerned with the Bear. Certainly less inscrutable. For recent inquiries into the lore and language of ancient Babylon have thrown much light on the relationships of the Orion fable.

There seems no reason to question the validity of Mr. Robert Brown's interpretation of the word by the Accadian *Ur-ana*, "light of heaven" ("Myth of Kirke," p. 146). But a proper name is significant only where it originates. Moreover, it is considered certain that the same brilliant star-group known to Homer no less than to us as Orion, was termed by Chaldeo-Assyrian peoples "Tammuz" (Lenormant, *Origines de l'Histoire*, t. i. p. 247), a synonym of Adonis. Nor is it difficult to divine how the association came to be established. For about 2000 B.C., when the Euphratean constellations assumed their definitive forms, the belt of Orion began to be visible before dawn in the month of June, called "Tammuz," because the death of Adonis was then celebrated. It is even conceivable that the heliacal rising of the asterism may originally have given the signal for that celebration. We can at any rate scarcely doubt that it received the name of "Tammuz" because its annual emergence from the solar beams coincided with the period of mystical mourning for the vernal sun.

Orion, too, has solar connexions. In the Fifth "Odyssey" (121-24), Calypso relates to Hermes how the love for him of Aurora excited the jealousy of the gods, extinguished only when he fell a victim to it, slain by the shafts of Artemis in Ortygia. Obviously, a sun-and-dawn myth slightly modified from the common type. The post-Homeric stories, too, of his relations with Œnopion of Chios, and of his death by the bite of a scorpion (emblematical of darkness, like the boar's tusk in the Adonis legend), confirm his position as a luminous hero (R. Brown, *Archæologia*, vol. xlvii. p. 352; "Great Dionysiak Myth," chap. x. § v.). Altogether, the evidence is strongly in favour of considering Orion as a variant of Adonis, imported into Greece from the East at an early date, and there associated with the identical group of stars which commemorated to the Accads of old the fate of Dumuzi (*i.e.* Tammuz), the "Only Son of Heaven."

It is remarkable that Homer knows nothing of stellar mythology. He nowhere attempts to account for the names of the stars. He has no stories at his fingers' ends of translations to the sky as a ready means of exit from terrestrial difficulties. The Orion of his acquaintance—the beloved of the Dawn, the mighty hunter, surpassing in beauty of person even the divinely-born Aloidæ—died and descended to Hades like other mortals, and was there seen by Ulysses, a gigantic shadow "driving the wild beasts together over the mead of asphodel, the very beasts which he himself had slain on the lonely hills, with a strong mace all of bronze in his hand, that is ever unbroken ("Odyssey," xi. 572-75). His stellar connexion is treated as a fact apart. The poet does not appear to feel any need of bringing it into harmony with the Odyssean vision.

The brightest star in the heavens is termed by Homer the "dog of Orion." The name *Seirios* (significant of sparkling), makes its *début* in the verses of Hesiod. To the singer of the "Iliad" the dog-star is a sign of fear, its rising giving presage to "wretched mortals" of the intolerable, feverish blaze of late summer (*opora*). The deadly gleam of its rays hence served the more appropriately to exemplify the lustre of havoc-dealing weapons. Diomed, Hector, Achilles, "all furnish'd, all in arms," are compared in turn, by way of prelude to an "*aristeia*," or culminating epoch of distinction in battle, to the same brilliant but baleful object. Glimmering fitfully across clouds, it not inaptly typifies the evanescent light of the Trojan hero's fortunes, no less than the flashing of his armour, as he moves restlessly to and fro ("Iliad," xi. 62-6). Of Achilles it is said:—

"Him the old man Priam first beheld, as he sped across the plain, blazing as the star that cometh forth at harvest-time, and plain seen his rays shine forth amid the host of stars in the darkness of night, the star whose name men call Orion's Dog. Brightest of all is he, yet for an evil sign is he set, and bringeth much fever upon hapless men. Even so on Achilles' breast the bronze gleamed as he ran" (xxii. 25-32).

In the corresponding passage relating to Diomed (v. 4-7), the *naïve* literalness with which the "baths of Ocean" are thought of is conveyed by the hint that the star shone at rising with increased brilliancy through having newly washed in them.

Abnormal celestial appearances are scarcely noticed in the Homeric poems. There are neither eclipses[1] of sun or moon, nor comets, nor star-showers. The rain of blood, by which Zeus presaged and celebrated the death of Sarpedon ("Iliad," xvi. 459, also xi. 54) might be thought to embody a reminiscence of a crimson aurora, frequently, in early times, chronicled under that form; but the portent indicated is more probably an actual shower of rain tinged red by a microscopic alga. An unmistakable meteor, however, furnishes one of the glowing similes of the "Iliad." By its help the irresistible swiftness and unexpectedness of Athene's descent from Olympus to the Scamandrian plain are illustrated.

"Even as the son of Kronos the crooked counsellor sendeth a star, a portent for mariners or a wide host of men, bright shining, and therefrom are scattered sparks in multitude; even in such guise sped Pallas Athene to earth, and leapt into their midst" ("Iliad," iv. 75-9).

In the Homeric verses the Milky Way—the "path of souls" of prairie-roving Indians, the mediæval "way of pilgrimage"[2]—finds no place. Yet its conspicuousness, as seen across our misty air, gives an imperfect idea of the lustre with which it spans the translucent vault which drew the wondering gaze of the Ionian bard.

The point of most significance about Homer's scanty astronomical notions is that they were of home growth. They are precisely such as would arise among a people in an incipient stage of civilisation, simple, direct, and childlike in their mode of regarding natural phenomena, yet incapable of founding upon them any close or connected reasoning. Of Oriental mysticism there is not a vestige. No occult influences rain from the sky. Not so much as a square inch of foundation is laid for the astrological superstructure. It is true that Sirius is a "baleful star"; but it is in the sense of being a harbinger of hot weather. Possibly, or probably, it is regarded as a concomitant cause, no less than as a sign of the August droughts; indeed the *post hoc* and the *propter hoc* were, in those ages, not easily separable; the effect, however, in any case, was purely physical, and so unfit to become the starting-point of a superstition.

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[1] Continued from p. 588.

[1] Görlitz finds a prediction of a solar eclipse at "Odyssey," xx. 357; but the expression appears quite indefinite and figurative.

[2] To Compostella. The popular German name for the Milky Way is still *Jakobsstrasse*, while the three stars of Orion's belt are designated, in the same connexion, *Jakobsstab*, staff of St. James.

The Homeric names of the stars, too, betray common reminiscences rather than foreign intercourse. They are all either native, or naturalised on Greek soil. The transplanted fable of Orion has taken root and flourished there. The cosmopolitan Bear is known by her familiar Greek name. Boötes is a Greek husbandman, variously identified with Arkas, son of Callisto, or with Ikaros, the luckless mandatory of Dionysos. The Pleiades and the Hyades are intelligibly designated in Greek. The former word is usually derived from *plein*, to sail; the heliacal rising of the "tangled" stars in the middle of May having served, from the time of Hesiod, to mark the opening of the season safe for navigation, and their cosmical setting, at the end of October, its close. But this etymology was most likely an after-thought. Long before rules for navigating the Ægean came to be formulated, the "sailing-stars" must have been designated by name amongst the Achaian tribes. Besides, Homer is ignorant of any such association. Now in Arabic the Pleiades are called *Eth Thuraiyâ*, from *therwa*, copious, abundant. The meaning conveyed is that of many gathered into a small space; and it is quite similar to that of the Biblical *kimah*, a near connexion of the Assyrian *kimtu*, family (R. Brown, "Phainomena of Aratus," p. 9; Delitzsch, "The Hebrew Language," p. 69). Analogy, then, almost irresistibly points to the interpretation of Pleiades by the Greek *pleiones*, many, or *pleios*, full; giving to the term, in either case, the obvious signification of a "cluster."

Of the Hyades, similarly, the "rainy" association seems somewhat far-fetched. They rise and set respectively about four days later than the Pleiades; so that, as prognostics of the seasons, it would be difficult to draw a permanent distinction between the two groups; yet one was traditionally held to bring fair, the other foul weather. There can be little doubt that an etymological confusion lay at the bottom of this inconsistency. "To rain," in Greek is *huein*; but *hus* (cognate with "sow") means a "pig." Moreover, in old Latin, the Hyades were called *Suculæ* ("little pigs"); although the misapprehension which he supposed to be betrayed by the term was rebuked by Cicero (*De Nat. Deorum*, lib. ii. cap. 43). Possibly the misapprehension was the other way. It is quite likely that "Suculæ" preserved the original meaning of "Hyades," and that the pluvious derivation was invented at a later time, when the conception of the seven stars in the head of the Bull as a "litter of pigs" had come to appear incongruous and inelegant. It has, nevertheless, just that character of *naïveté* which stamps it as authentic. Witness the popular names of the sister-group—the widely-diffused "hen and chickens," Sancho Panza's "las siete cabrillas," met and discoursed with during his famous aërial voyage on the back of Clavileño, the Sicilian "seven dovelets,"—all designating the Pleiades. Still more to the purpose is the Anglo-Saxon "boar-throng," which, by a haphazard identification, has been translated as Orion, but which Grimm, on better grounds, suggests may really apply to the Hyades (*Teutonic Mythology*, trans. by J. S. Stallybrass, vol. ii. p. 729). It is scarcely credible that any other constellation can be indicated by a term so manifestly reproducing the "Suculæ" of Latin and Sabine husbandmen.

The Homeric scheme of the heavens, then, (such as it is), was produced at home. No stellar lore had as yet been imported from abroad. An original community of ideas is just traceable in the names of some of the stars; that is all. The epoch of instruction by more learned neighbours was still to come. The Signs of the Zodiac were certainly unknown to Homer, yet their shining array had been marshalled from the banks of the Euphrates at least 2000 years before the commencement of the Christian era. Their introduction into Greece is attributed to Cleostratus of Tenedos, near, or shortly after, the end of the sixth century B.C. By that time, too, acquaintance had been made with the "Phœnician" constellation of the Lesser Bear, and with the wanderings of the planets. Astronomical communications, in fact, began to pour into Hellas from Egypt, Babylonia, and Phœnicia about the seventh century B.C. Now, if there were any reasonable doubt that "blind Melesigenes" lived at a period anterior to this, it would be removed by the consideration of what he lets fall about the heavenly bodies. For, though he might have ignored formal astronomy, he could not have remained unconscious of such striking and popular facts as the identity of Hesperus and Phosphorus, the Sidonian pilots' direction of their course by the "Cynosure," or the mapping-out of the sun's path among the stars by a series of luminous figures of beasts and men.

Thus the hypothesis of a late origin for the "Iliad" and "Odyssey" is negatived by the astronomical ignorance betrayed in them. It has, however, gradations; whence some hints as to the relative age of the two epics may be derived. The differences between them in this respect are, it is true, small, and they both stand approximately on the same astronomical level with the poems of Hesiod. Yet an attentive study of what they have to tell us about the stars affords some grounds for placing the "Iliad" the "Odyssey," and the "Works and Days" in a descending series as to time.

In the first place, the division of the month into three periods of ten days each is unknown in the "Iliad," is barely hinted at in the "Odyssey," but is brought into detailed notice in the Hesiodic calendar. Further, the "turning-points of the sun" are unmentioned in the "Iliad," but serve in the "Odyssey," by their position on the horizon, to indicate direction; while the winter solstice figures as a well-marked epoch in the "Works and Days." Hesiod, moreover, designates the dog-star (not expressly mentioned in the "Odyssey") by a name of which the author of the "Iliad" was certainly ignorant. Besides which an additional constellation (Boötes) to those named in the "Iliad" appears in the "Odyssey" and the "Works and Days"; while the title "Hyperion," applied substantively to the sun in the "Odyssey," is used only adjectivally in the "Iliad." Finally, stellar mythology begins with Hesiod; Homer (whether the Ionian or the Ithacan) takes the names of the stars as he finds them, without seeking to connect them with any sublunary occurrences.

To be sure, differences of place and purpose might account for some of these discrepancies, yet their cumulative effect in fixing relative epochs is considerable; and, even apart from chronology, it is something to look towards the skies with the "most high poet," and to retrace, with the aid of our own better knowledge, the simple meanings their glorious aspect held for him.

A. M. CLERKE

ON ICE AND BRINES[1]

I.

THE composition of the ice produced in saline solutions, and more particularly in sea-water, has frequently been the object of investigation and of dispute. It might be thought that to a question of whether ice so formed does or does not contain salt, experiment would at once give a decisive answer. Yet, relying on experiment alone, competent authorities have given contradictory answers. All agree that ice, whether formed artificially in the laboratory by freezing sea-water, or found in nature as one of the various species of sea-water ice, retains, in one form or another, and with great tenacity, the salt existing in solution in the water. The question at issue is whether this salt is to be attributed to the solid matter of the ice or to the liquor mechanically adhering to it, from which it is impossible to free it. Most bodies, and especially those which take a crystalline form, are easily purified and freed from all suspected foreign matter, with a view to analysis, by the simple operation of washing and drying. It is impossible to wash the crystals, formed by freezing a saline solution, with distilled water, because they melt at a temperature below that at which distilled water freezes. The effect of the addition of a small quantity of distilled water to a quantity of saline ice is at first the anomalous one, that what was a wet sludge is transformed into a dry crystalline powder. It is of course impossible to dry the ice by heat, and to do so by more intense freezing would be begging the question. The experimental difficulties therefore account for some of the divergence of opinion on the subject. The mixed character of the substances examined has also much to do with it. As a rule it may be said that those investigators who have confined their observations to the laboratory have concluded that the ice forming when saline solutions of moderate concentration, including sea-water, are frozen, is pure ice, and the salt from which it is impossible to free it entirely belongs to the mother-liquor, while those who have collected and examined sea-water ice in high latitudes have come to the opposite conclusion.

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[1] Paper read before the Royal Society of Edinburgh, by J. Y. Buchanan on March 27 last.

During the Antarctic cruise of the *Challenger* I made a number of observations on the sea-water ice found in those regions, and relying principally on the fact that the melting temperature of the ice was markedly lower than that of fresh-water ice, and that it was impossible by any of the ordinary means familiar to chemists for freeing crystals from adhering mother-liquor to materially reduce its salinity, I came to the conclusion that the ice forming in freezing sea-water is not a mixture of pure ice and brine, but that it contains the salt found in it in the solid state either as a crystalline hydrate or as the anhydrous salt, but most probably as a hydrate. In dealing with this subject, Dr. Otto Pettersson ("Water and Ice," p. 302) quotes my observations, and also rejects the view that "sea-ice is in itself wholly destitute of salts, and only mechanically incloses a certain quantity of unfrozen and concentrated sea-water." He founds his belief on the fact that numerous analyses of specimens of sea-water ice have shown that the constitution of the saline contents of different specimens of ice differs for each specimen, and is always different from that of the saline contents of sea-water. Were the salinity due to inclosed unfrozen and concentrated sea-water, we "ought to find by chemical analysis exactly the same proportion between Cl, MgO, CaO, SO_3, &c., in the ice and in the brine as in the sea-water itself." He adduces numerous examples of analyses of specimens of sea-water ice from the Baltic and from the Arctic Seas to show that this is not the case. Calling the percentage of chlorine in each case 100, he found in various sea-waters the percentage of SO_3 vary from 11 49 to 11·89. In specimens of sea-water ice it varied from 12·8 to 76·6, and in brines separating from the ice and remaining liquid at −30° C. it varied from 1·14 to 1·16.

This argument appears conclusive. In order to explain all the phenomena observed in connexion with sea-water ice he cites Guthrie's investigations, which went to show that, in freezing saline solutions, under a certain concentration, pure ice is formed at a temperature which falls from 0° C., when the amount of salt dissolved is infinitely small, to a certain definite temperature when the solution contains a certain definite percentage of salt. Further abstraction of heat then produces solidification of the solution as a whole, in the form of a crystalline hydrate, of constant freezing- and melting-point. To such hydrates, Guthrie gave the name of cryohydrates. Pettersson quotes the following as being particularly applicable to the case of sea-water :—

The cryohydrate of		Contains per cent. of water		Solidifies at ° C.
NaCl	...	76·39	...	−22
KCl	...	80·00	...	−11·4
$CaCl_2$	...	72·00	...	−37·0
$MgSO_4$	...	78·14	...	− 5·0
Na_2SO_4	...	95·45	...	− 0·7

And he refers more particularly to the cryohydrate of Na_2SO_4 forming and melting at −0°·7.

Now the bearing of Guthrie's experiments is to show that, while at sufficiently low temperatures, and with suitable concentration, the water will solidify along with one or other of the salts in solution, until this low temperature and high concentration are attained, pure ice must be the result of freezing.

The abnormal phenomena attending the formation and the melting of ice in saline solutions and sea-water, find a natural explanation in an observation which I have frequently quoted, and which Dr. Pettersson mentions in a footnote at p. 318, namely, that "a thermometer immersed in a mixture of snow and sea-water which is constantly stirred indicates −1°·8 C." If this is true, it is clear that my melting-point observations proved nothing. On repeating the experiment I found it confirmed, and took the opportunity this winter of investigating the matter more closely. The paper read before the Royal Society of Edinburgh contains the first portion of the results. It deals with the subject under two heads, namely, (*a*) the temperature at which sea-water and some other saline solutions freeze, and the chemical constitution of the solid and the liquid into which they are split by freezing; and (*b*) the temperature at which pure ice melts in sea-water and in a number of saline solutions of different strengths.

(*a*) The freezing experiments were limited to sea-water and solutions of NaCl comparable with sea-water.

Chloride of Sodium.—Four solutions were used, and they were intended to contain 3, 2·5, 2, and 1·5 per cent. NaCl respectively. Forty grammes of this solution, in a suitable beaker, were immersed in a freezing mixture of such composition as to give a temperature from 2° to 2°·5 C. below the freezing temperature expected. The temperature at which ice began to form (if necessary after adding a minute splinter of ice) was noted, and the freezing was allowed to continue with constant stirring till the temperature had fallen 0°·2 C. A specimen of the mother-liquor was removed, and the chlorine in it determined; the chlorine in the original solution had been determined before. The beaker was then removed from the freezing bath and allowed to melt. The temperature in all cases rose during melting exactly as it had fallen during freezing. In the following table are given the means of the temperature at which ice began to form in the original solution, and that of the liquid when the sample of brine was taken, and the means of the chlorine found in the original solution and in the brine sample

Mean Freezing Temp.	−1°·875 C.	−1°·63	−1°·30	−0°·975
Mean per cent. Cl.	1·87	1·60	1·30	0·98

It will be seen that, in the dilute solutions experimented with, the percentage of chlorine expresses, in terms of the Centigrade scale, the lowering of the freezing-point of the solution.

Sea-Water.—Similar experiments were made with sea-water of different degrees of concentration. In sea-water from the Firth of Clyde containing 1·84 per cent. of chlorine, ice forms at −1°·9 C. The following results are from means of closely-agreeing results:—

Freezing temperature	−2°·0	−1°·5	−1°·0	−0°·5
Per cent. chlorine	1·94	1·445	0·963	0·475
Difference .. … …	0·06	0·055	0·037	0·025

Sea-water resembles a chloride of sodium solution, containing the same percentage of chlorine, and the resemblance is closer the greater the dilution. When the beaker was removed from the freezing-bath, the temperature rose during melting as it had fallen during freezing. In these experiments, which had for their object the determination of the temperature at which the crystals melted, as well as that at which they began to form in the water, it was impossible to remove a sample for analysis large enough to enable the sulphuric acid to be determined in it.

For this purpose a series of observations were made, using quantities of 300 grammes of sea-water. Freezing was continued usually until the temperature had fallen 0°·3 C. below that at which crystals began to form. The mother-liquor was then separated from the crystals by means of a large pipette with fine orifice, before removing the beaker from the freezing bath. The magma of crystals was then brought rapidly on a filter and drained by means of the jet pump. The ice, thus drained, was then melted, and the three fractions were analysed. In the following table (I.) the results of four experiments are given. In the one column (W) will be found the weight of the original water taken and of the fractions into which it was split in freezing; in the other (R) will be found the ratio of SO_3 to Cl found by analysis, the chlorine being set down as 100; thus, in I, the percentage of chlorine found in the crystals, melting at the lowest temperature, was 1·497, and that of the SO_3, 0·174; the ratio (R) is therefore 11·62.

TABLE I.—*Freezing sea-water—Analyses of fractions.*

No. of Experiment.	I.		II.		III.		IV.	
Nature of Water...	Forth 100 °/.		Mother liquor		Clyde 100 °/.		Clyde 50 °/.	
	W.	R.	W.	R.	W.	R.	W.	R.
Original water	300	11·83	90	11·67	300	11·58	300	11·21
Mother-liquor	170·6	11·67	—	11·83	102	11·57	78	11·67
Drainings			—	—	94	11·56	109	—
Crystals	106	11·62	23	11·22	97	11·67	106	11·44
„	22·5	11·11	—	—	—	—	—	—

It will be seen that the ratios (R) found for mother-liquor, drainings, and ice agree with one another quite as closely as those found in samples of pure sea-water from different localities. It is to be remembered that in these experiments the water was frozen *gently*, that is, the rate of abstraction of heat was low, the temperature of the freezing bath being regulated so as to be about 2° C. below the freezing temperature of the solution. Much of the error and uncertainty about the freezing of saline solutions arises from the violence of the methods employed. Judging then by the constancy of the relation of the percentage of Cl to SO_3 we see that in sea-water, frozen at moderate temperatures, the composition of the saline contents of the original water, the mother-liquor and the ice is identical; and we are justified in concluding that it is probable that the saltness of the ice is due to unfrozen and concentrated sea-water adhering to it. Ice forming in even very weak saline solutions closely resembles snow (which is ice forming in air), and has the same remarkable power of retaining mechanically several times its weight of water or brine.

A strict account was kept of the heat removed from the sea-water while the freezing was going on. In Table II. will be found the number of heat-units (gramme ° C.) removed during the freezing in the case of Nos. III. and IV.; and this number, divided by 79·25, gives the weight of pure ice, which could have been formed at 0° C. by the removal of heat.

TABLE II.—*Calculation of ice formed.*

		III.	IV.
Weight of original water (grammes)	W	300	300
Per cent. Cl in ditto	c	1·836	0·923
Per cent. Cl in mother-liquor	K	2·212	1·153
Weight of mother-liquor $W\frac{c}{K}=$	L	249·0	239·3
Weight of ice $W-L=$	I	51·0	60·7
Mean freezing temperature (° C).....................	...	−2°·05	−1°·05
Heat abstracted (grammes ° C)	...	4230	5193
Equivalent ice formed (grammes)	...	53·4	65·5

Sea-water, like other saline solutions, is easily cooled several degrees below its freezing-point before crystals begin to form. While cooling down to and below what was known to be its freezing-point, simultaneous observations of the temperature of the sea-water and the freezing bath were made from half-minute to half-minute. From these observations, the rate of abstraction of heat for different differences of temperature of sea-water and bath was found. At a given moment a minute splinter of ice (weighing much less than a drop of water) was introduced. Crystals immediately began to form, and the temperature rose in from ten to fifteen seconds to the freezing-point. During the freezing the temperatures of bath and sea-water were observed at regular intervals. The heat removed is thus made up of that eliminated during the few seconds when freezing began and the temperature rose to the freezing-point, which is found by multiplying the rise of temperature by the weight of liquid, and that removed during the subsequent cooling, which is deduced from the duration of the operation and the rate of loss of heat observed before freezing commenced. The specific heat of the solution is taken as unity. In the table are further given the weight of the sea-water used, the percentage of chlorine in the original water and in the mother-liquor, the weight of the mother-liquor on the assumption that it contains all the salt of the original water, and, by difference, the weight of the ice formed. The agreement between the two quantities of ice formed as calculated by the different methods is as close as could be expected.

It has thus been shown that the composition of the saline contents of the ice formed as above described is the same as that of the original water, and this of itself is almost conclusive that the salt is contained in adhering brine and not as a solid constituent of the ice. Assuming that this is so the amount of ice formed as deduced from the composition of the mother-liquor agrees well with the amount deduced from the thermal exchange taking place during the freezing.

It has, moreover, been proved by Guthrie, Rüdorff, and others, that, in solutions of the salts occurring in sea-water, ice does separate out at first, and continues to separate out until the concentration has become many times greater than that of sea-water. Assuming that in sea-water all the chlorine is united to sodium, 87 per cent. of the water would have to be removed as ice before a cryohydrate would form, and if it contained nothing but sulphate of soda in the proportion corresponding to the sulphuric acid formed in it, over 90 per cent. of the water would have to go as ice, before the cryohydrate would be formed.

In my experiments, about 15 per cent. of the weight of the water was frozen out as ice, causing a lowering of freezing-point by 0°·3 C. In nature it is probable that the ice forming at the actual freezing surface does so at an almost uniform temperature, the local concentration produced by the formation of a crystal of ice being immediately eliminated by the mass of water below. In the interstices of the crystals there will be retained a weight of slightly concentrated sea-water at least as great as that of the ice crystals. These retain the brine in a meshwork of cells, and, as the thickness of the ice covering increases, and the freezing surface becomes more remote, the ice and the brine become more and more exposed to the atmospheric rigours of the Arctic winter. The brine will continue to deposit ice until its concentration is such that, for example, the cryohydrate of NaCl is ready to separate out. It probably will separate out until it comes in conflict with, for instance, the chloride of calcium or the chloride of magnesium, which will retain some of the water, without solidifying, even at the lowest temperatures. At the winter quarters of the *Vega* brine was observed oozing out of sea-water ice and liquid at a temperature of – 30° C. It was very rich in calcium and especially magnesium chlorides. In fact, *it is probably quite impossible by any cold occurring in nature to solidify sea-water.*

The residual and unfreezable brine which remains in considerable quantity liquid when sea-water is frozen, must also remain in greater or less quantity when fresh water is frozen. All natural waters, including rain-water, contain some foreign and usually saline ingredients. If we take chloride of sodium as the type of such ingredients, and suppose a water to contain a quantity of this salt equivalent to one part by weight of chlorine in a million parts of water, then we should have a solution containing 0·0001 per cent. of chlorine, and it would begin to freeze and to deposit pure ice at a temperature of –0°·0001 C.; and it would continue to do so until, say, 999,000 parts of water had been deposited as ice. There would then remain 1000 parts of residual water, which would retain the salt, and would contain, therefore, 0·1 per cent. of chlorine, and would not freeze until the temperature had fallen to –0°·1 C. This water would then deposit ice at temperatures becoming progressively lower, until, when 900 more parts of ice had been deposited, we should have 100 parts residual water, or brine as it might now be called, containing 1 per cent. of chlorine, and remaining liquid at temperatures above – 1°·0 C. When 90 more parts of ice had been deposited, we should have 10 parts of concentrated brine containing 10 per cent. of chlorine and remaining liquid at – 10° C. In the case imagined, we assume the saline contents to consist of NaCl only, and with further concentration the cryohydrate would no doubt separate out and the mass become really solid. On reversing the operations, that is, warming the ice just formed, we should, when the temperature had risen to – 10° C., have 999,990 parts ice and 10 brine containing 10 per cent. chlorine. Now, owing to the remarkable fact (which is dealt with at length in the second part of the paper) that pure ice, in contact with a saline solution, melts at a temperature which depends on the nature and the amount of the salt in the solution, and is identical with the temperature at which ice separates from a solution of the same composition on cooling, the brine liquefies more and more ice at progressively rising temperatures, until, as before, when the temperature of the mass has risen to –0°·1 C., it consists of 999,000 parts of ice and 1000 parts of liquid water, containing 1 part of chlorine. The remainder of the ice will melt at a temperature gradually rising from –0°·1 C. to 0° C.

The consideration of this example furnishes an easy explanation of the anomalous behaviour of ice, formed from anything but the very purest distilled water, in the neighbourhood of its melting-point. This subject has been studied with great care and thoroughness by Pettersson. The apparent expansion of all but the very purest ice, when cooled below 0° C., is ascribed by him in part to solid saline contents of the ice which exercise a disturbing and unexplained influence on its physical properties. Viewed in the light of the fact that the presence of even the smallest quantity of saline matter in solution prevents the formation of ice at 0° C. and promotes its liquefaction at temperatures below 0° C., we see that this apparent expansion of the ice on cooling is probably due to the fact that we are dealing not with homogeneous solid ice but with a mixture of ice and saline solution. As the temperature falls this solution deposits more and more ice and its volume increases. But the increase of volume is due to the formation of ice out of water and not to the expansion of a crystalline solid already formed.

In Table III. are given the volumes occupied by the ice (with inclosed brine) formed by freezing 100,000 c.c. (at 0° C.) of a water containing chloride of sodium equivalent to 7 grammes chlorine in 1,000,000 cubic centimetres (at 0° C.).

TABLE III.—*Water containing 7 parts Cl in* 1,000,000.

Temp. ° C.	Water frozen. c.c.	Ice formed. c.c.	Brine remaining. c.c.	Ice and Brine. c.c.	Pettersson III. Vol. of ice at *T*. c.c.	Diff.
T	V_1	v_1	V_2	v_2	P	$P - v_2$
– 0·07	99000	107979	1000	108979	108980	1
– 0·10	99300	108306	700	109006	109007	1
– 0·15	99533	108561	467	109028	109038	10
– 0·20	99550	108687	350	109037	109048	11
– 0·40	99825	108879	175	109054	109057	3

The volume of the ice formed on freezing this water is compared with that observed by Pettersson in freezing a sample of the distilled water in ordinary use in the laboratory.

It will be seen that the volumes observed by Pettersson agree very closely with those calculated for a water containing 7 parts of chlorine in a million.

The irregularities in the melting-points of bodies like acetic acid, to which Pettersson refers, are without doubt due to a perfectly similar cause.

Also the very low latent heat observed by Pettersson for sea-water is to be explained by the fact that the salt retains a considerable proportion of the water in the liquid state even at temperatures many degrees below the freezing-point of distilled water.

The *plasticity* of ice and the motion of glaciers receive a simple and natural explanation when we see, as in Table III., that, if the water from which this ice is produced contains no more than 7 parts of chlorine per million, it will, in the process of thawing, when the temperature has risen to –0°·07 C., consist to the extent of 1 per cent of its mass of liquid brine or water. The water considered in Table III. is certainly not less free from foreign ingredients than rain or snow. It follows, therefore, that a glacier, in a climate where the temperature is for the greater part of the year above 0° C., must have a tendency to *flow*, owing to the power of saline solutions to deposit ice and to dissolve it at temperatures below 0° C.

(*To be continued.*)

NOTES

THE Endowed Schools Committee, of which Sir Lyon Playfair is Chairman, after sitting for a year and a half, have agreed to their Report. This is not yet issued, but it is known that the Committee have reported in favour of endowed schools being in future largely used for the promotion of scientific and technical education. The Report also recommends that local authorities should be authorised to employ local rates for founding or contributing to laboratories and workshops in such schools in order to promote practical scientific education.

LAST week an important Conference was held at Oxford for the consideration of questions connected with the University Extension Scheme. The meetings were attended by many members of the University, local delegates, and others interested in this method of University development. There can be no doubt whatever as to the good work done by University Extension lecturers. Unfortunately, however, it is hard to obtain the funds which are necessary for the complete success of the experiment. One speaker urged that "the University should lead the way by the creation of a Jubilee Fund, and so stimulate local efforts." To this Prof. Rogers replied that the University was "positively poor." Prof. Rogers added that the result of an appeal to London Companies "had not been favourable," but that the Charity Commissioners might perhaps be willing to do something for the movement.

THE American Exhibition to be opened in the Earl's Court neighbourhood on May 9 will contain much that ought to be of scientific interest. The large encampment of American Indians will be found to contain a great variety of types, and ought to prove attractive to ethnologists. The machinery department will contain many illustrations of the successful applications of science, especially in the section directed to machines for the production and application of electricity. Under mining and metallurgy there will be a large collection of minerals, ores, and stones, besides specimens of metallurgical products. In the department of education and science will be found illustrations of the varied educational appliances and apparatus used in the United States, exhibits from the many institutions and organisations which exist in the States, and a very varied collection of scientific and philosophical instruments.

A NEW scientific journal—*Centralblatt für Physiologie*—has made its appearance this month in Germany. It is edited by Dr. S. Exner, of Vienna, and Dr. J. Gad, of Berlin, who have the advantage of the co-operation of the Berlin Physiological Society. The journal will be issued once a fortnight.

A MONUMENT to Galileo has been erected in Rome, on the Via Pincio, fronting the old Medici Palace, now occupied by the French Embassy, where he was kept a prisoner, in 1637, during his prosecution by the Inquisition. The monument consists of a column with a pedestal, on which is the following inscription in the Italian language:—"Erected in memory of Galileo Galilei, who was kept a prisoner in this Palace, for having seen that the earth moves round the sun."

DR. R. MÜLLER publishes, in the April number of *Mittheilungen aus dem Gebiete des Seewesens*, the results of an investigation as to whether or not the popular idea of equinoctial gales, the existence of which was contested some time ago as regards this country by Mr. Scott (NATURE, vol. xxx. p. 353), holds good for the Adriatic Sea. For this purpose the hourly records of the anemometer at Pola, from 1876-86, were used. During this period strong winds or gales were registered on 607 days, 63 per cent. of which occurred in the winter season (October to March). The months with least wind were naturally June and July; then a tolerably regular increase in the number of days with stormy winds took place till the end of January. A considerable decrease occurred in Febuary, while March had the greatest number of stormy days. The result arrived at is that, for the Adriatic, no important influence can be attributed to the equinoctial seasons, especially during the autumnal equinox. Dr. Müller also quotes a similar investigation made by the *Deutsche Seewarte*, for the years 1878-83, for the German coasts, with nearly similar results; the percentage of storms for the winter season being 80 per cent., and for the summer season only 20 per cent. The greatest number of storms occurred in November and December, March having 14 per cent., and September 3 per cent. only.

AT a recent meeting of the Italian Meteorological Society, reported in its *Bollettino mensuale* for February last, the Committee enumerated the meteorological stations lately established by it abroad, viz. one in Tripoli, two in the Argentine Republic, three in Uruguay, one in Colombia (South America), and one in Mexico, and notified their intention of shortly establishing others on the Patagonian coasts of the Pacific, and in some of the islands near Cape Horn. Among other matters discussed was the proposed suppression of the *Bollettino decadico*—which has reached its fourteenth yearly volume, and which contains the observations made at ordinary stations, and at stations in the Alps and Apennines, for decades—as the increasing work of the Society renders its publication difficult. The monthly means of these observations appear in the *Bollettino mensuale*, but the last published are for March 1886.

ON the night of April 12, at about 11.30, a brilliant meteor was seen in Værdalen, in Norway. It appeared in the east, and went in a southerly direction. At first the colour was a pure white, but during the progress of the meteor it changed into green and yellow. The lustre of the body was very bright, and its greatest apparent size was about that of an ordinary plate. On the meteor disappearing from view behind a mountain ridge, a sudden brilliancy seemed to indicate that it had burst into fragments, but no detonation was heard. It left a faint trail of smoke behind, about a few yards in length, which remained for a few seconds in the sky, then disappeared.

ON April 13 a shock of earthquake was felt at Lisbon, and in Malta and Sicily. Considerable alarm was caused in Jersey at a few minutes past 3 o'clock on the morning of April 21 by a slight shock of earthquake, the direction of which was from south-west to north-west. There was so loud a noise that at first some persons fancied guns were being fired. About the same time a smart shock of earthquake was experienced in Guernsey. A decided tremor of the earth, lasting about three seconds, and accompanied by a rumbling noise, was felt in all parts of the island.

THE death is announced of Dr. Nathaniel Lieberkühn, Professor of Anatomy at the University of Marburg, on April 14, at the age of sixty-five; and of Herr J. B. Obernetter, well known by his researches in photographic chemistry, on April 13, at the age of forty-seven.

A NEW GUINEA Exhibition will shortly be opened at Bremen.

MESSRS. R. FRIEDLÄNDER AND SON, of Berlin, send us the first quarterly list for 1887 of their new publications. It includes many valuable works in natural history and the exact sciences.

THE scheme for the formation of a North Sea Fisheries Institute is still under the consideration of the National Fish-Culture Association and of various local authorities, by whom efforts are being made to secure the necessary funds. It is proposed to form a Fish-Culture Station at Cleethorpes, and schools at Grimsby, and to carry out scientific observations wherever the conditions seem to be most favourable. Particular attention is to be given to the culture of the oyster and cod. A meeting has lately been held in London to advance the undertaking, and another is to take place shortly at Grimsby for the same purpose.

THE list of elements whose atomic weights have been determined with great accuracy has just received two valuable additions, for although one of them, silicon, is by no means rare in its occurrence in nature, and the other, gold, is neither among the most recently discovered of metals nor rare from the chemist's point of view, yet past determinations of the atomic weights of these important elements have resulted in leaving the subject enshrouded with considerable ambiguity. Prof. Thorpe and Mr. J. W. Young, who have recently determined the atomic weight of silicon, used for this purpose the tetrabromide $SiBr_4$, and, instead of the small quantities of material used by other experimenters, prepared upwards of half a kilogramme of this substance by passing bromine vapour over a heated mixture of pure silicon dioxide and charcoal. The product was rectified in an atmosphere of nitrogen, and portions for analysis were collected in bulbs without exposure to air; these bulbs were then broken in bottles containing pure water, and in each case the resulting turbid liquid was very slowly evaporated to complete dryness, the relation of the weight of silicon dioxide obtained to that of the tetrabromide used forming the basis of the calculations. The result of this long and difficult series of analyses fixes the atomic weight of silicon as 28·332. The instability of the salts of gold has long been a stumbling-block in the way of obtaining accurate determinations of its atomic weight, but Prof. Thorpe and Mr. A. P. Laurie have overcome this difficulty by utilising the double bromide of gold and potassium, which they prepared by digesting together pure gold, bromine, potassium bromide, and water, the salt being afterwards subjected to several recrystallisations. An unknown quantity of the pure salt was then carefully ignited, and the mixture thus obtained, of potassium bromide and gold, weighed; after removal of the potassium bromide by washing, the weight of residual gold obtained in each analysis furnished data from which one set of values averaging 196·876 was obtained. The bromine in the potassium bromide as determined by titration with silver nitrate gave a second averaging 196·837, and the weight of silver bromide formed yielded a third series of numbers, averaging 196·842. Taking Stas's value for oxygen at 15·96 the atomic weight of gold is fixed by these analyses at 196·85; but if, along with Mendelejeff, we consider oxygen 16, then gold becomes 197·28, and it is interesting to note that Mendelejeff considered the old value, 195·2, to be too low, there being no place in the periodic system for an element of this atomic weight having the properties of gold. Hence the result of the present determination has been to place gold in its proper position in the periodic classification. Prof. Krüss has just published (*Ber. Deut. Chem. Ges.* No. 2, 1887) the results of a determination of the atomic weight of gold recently made by him, according to which the probable value is 196·64. The method employed, however, was slightly different from the above, a weighed quantity of the double bromide itself being used as the basis of a portion of the analyses.

IN a paper on Electric Locomotion, read before the Society of Arts on the 20th inst., Mr. Reckenzaun pointed out that in the United States and on the European continent there are dozens of electric tramways at work to the satisfaction of everybody, while we have few opportunities of testing their advantages in England. Yet England is "the home of the dynamo-machine, the country where the electromotor has found its highest development." We have been outstripped in this matter chiefly because an Act of Parliament must be obtained before electric cars can be run, and every company has to apply for it separately.

THE other day M. Émile Rivière discovered a prehistoric station in the wood of Chaville, to the right of the Versailles road, in a part where there are comparatively few trees. Some flint implements and weapons were found, and a fragment of coarse pottery without ornament.

AT the February meeting of the Pekin Oriental Society, Mr. Owen read a paper on animal worship amongst the Chinese. He referred at considerable length to the worship paid to the fox, weasel, hedgehog, and snake, to which at Tientsin is added the rat. The first four are called the immortals. These deified animals seem to usurp the entire attention of the people, even to the exclusion of the Buddhist and Taoist gods. Dr. Edkins quoted from the Chinese to prove that animal worship was unknown in ancient times, while Dr. Dudgeon pointed out that it was a mistake to suppose that animal worship was confined to the four animals mentioned. The horse, cow, dog, insects, dragon, lion, &c., are worshipped. In the fifth month the centipede, lizard, scorpion, frog, and snake—the five poisonous animals, as they are called—are also objects of worship. Dr. Martin disputed Dr. Edkin's theory that animal worship was unknown in ancient times because it was not mentioned by Confucius; he (Dr. Martin) believed that it existed to a much greater extent in ancient than in modern times. From the observations of other speakers it is clear that animal worship occupies a very large place in Chinese superstitious observances.

THERE was an interesting Geographical Exhibition in connexion with the meeting of the Deutsche Geographentag at Carlsruhe on the 13th inst. An historical department showed the development of cartography, all the exhibits being from Baden and Würtemberg. Water-colour sketches by the well-known African traveller Paul Reichard, and some fine oil sketches by the painter Hellgreve, who was sent to East Africa by the Artists' Club at Berlin, were exhibited. In the plant department were shown cotton plants found growing wild in Togoland, wild coffee and sugar-cane from Cameroons, and tobacco and various seeds from the gardens of the German East African Society.

AN interesting case of "colour-hearing" was recently reported to a meeting of the Société de Psychologie Physiologique. In this case, colour-hearing was an hereditary peculiarity, transmitted from father to daughter.

THE third volume of the "Grande Encyclopédie" has just been published It is expected that thirty volumes will probably be required to complete this great work. Each volume consists of 1200 quarto pages in two columns, printed in small type.

EVERYONE who takes an interest in zoology is probably acquainted with the "Zoological Record," which consists of an annual volume containing a summary of the work done in the various branches of zoology. It was begun in 1865, and for the first six years was published by Mr. Van Voorst. For the last sixteen years it has been carried on by an Association of zoologists called the "Zoological Record" Association, who have received assistance in the shape of grants from the Government Fund of the Royal Society, the British Association, and the Zoological Society of London. The "Zoological Record" Association, having been lately unsuccessful in obtaining the continuance of some of these grants, and being unwilling to carry on the publication of the "Record" any longer, have agreed, upon certain conditions, to transfer their whole stock to the Zoological Society of London, who, for the future, have determined to carry on this most useful publication. The "Zoological Record" will, therefore, beginning with the volume for the present year (which will contain the record of the zoological literature of 1886), be published by the Zoological Society of London, under the management of a Committee of the Council of that Society, and will be edited by Mr. F. E. Beddard, the Society's Prosector. The "Record" will be published for the Society by Messrs. Gurney and Jackson at the former price of 30*s.* per volume. But all members of the Society will have the privilege of receiving it, including the cost of delivery, at a subscription price of 20*s.* per annum. This subscription will be due on July 1 in every year, and the privilege of subscription will be forfeited unless the amount be paid before December 1 following. The Zoological Society, having purchased the entire stock of the "Zoological Record," are now able to supply complete sets of the first twenty-two volumes at the price of 5*l.* 10*s.*, that is 5*s.* per volume. Volumes of any single year can likewise be supplied at 10*s.* per volume. Learned Societies and Institutions and members of the former "Zoological Record" Association will be permitted to subscribe to the "Record" on the same conditions as are accorded to members of the Zoological Society.

It is intended to form a class, including beginners and advanced students, this summer, in the same way as last year, for the study of geology near London, to be conducted on Saturday afternoons, by Prof. H. G. Seeley, F.R.S., King's College. Applications for tickets should be made at once to the Hon. Secretary, W. W. R. May, 16 Bethune Road, Manor Road, Stoke Newington, N.

The additions to the Zoological Society's Gardens during the past week include two Rhesus Monkeys (*Macacus rhesus* ♂ ♀) from India, presented by Mr. W. F. Lock; a Common Fox (*Canis vulpes* ♂) British, presented by Mr. Isaac Bell, Jun.; a Bosch-bok (*Tragelaphus sylvaticus* ♂) from South Africa, presented by Capt. Travers; a Vulpine Phalanger (*Phalangista vulpina* ♂) from Australia, presented; two Turkey Vultures (*Cathartes aura*) from the Falkland Islands, presented by Mr. James H. Moore; a Common Viper (*Vipera berus*), British, presented by Mr. S. E. Gunn; a Chinese Lark (*Melanocorypha mongolica*) from China; two Common Rheas (*Rhea americana* ♂ ♀) from the Argentine Republic, received in exchange; a Burrhel Wild Sheep (*Ovis burrhel*); two White-backed Pigeons (*Columba leuconota*) from the Himalayas; a Hodgson's Partridge (*Perdix hodgsoniæ*) from Bootan, purchased; four Long-fronted Gerbilles (*Gerbillus longifrons*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

Barnard's First and Second Comets 1887.—Comet *c* 1887, discovered by Mr. Barnard on January 23, has now attained a high declination, and, though faint, may possibly be observed within the next few days. Dr. H. Kreutz gives the following ephemeris for it for Berlin midnight, derived from Dr. H. Oppenheim's elements:—

1887	R.A. h. m. s.	Decl. ° ′	log Δ	log *r*
April 27	1 12 46	65 40·4	0·4742	0·3989
29	1 21 49	65 52·7		
May 1	1 30 47	66 3·8	0·4827	0·4057
3	1 39 38	66 13·7		
5	1 48 22	66 22·5	0·4910	0·4125

Correction of ephemeris for April 15, in R.A. + 3m. 46s.; in Decl. *nil*.

The second comet discovered by Mr. Barnard, viz. Comet *d* 1887, discovered February 16, is scarcely likely to be seen further. Its present brightness is but one-seventh of that which it had on March 11, and having almost exactly the same R.A. as the sun it scarcely frees itself from the twilight. An ephemeris for its course throughout the month of April, based on Dr. Palisa's elements, was given by Dr. Kreutz in the *Astronomische Nachrichten*, No. 2777, and in the *Observatory* for April; but the probability of its being observed after the April full moon had passed away, except with very powerful instruments, was very slight.

Probable Re-discovery of Hesperia.—Dr. R. Luther detected a minor planet on April 11 which it appears probable is no other than the lost Hesperia, No. 69, as the magnitude and motion resembled those of the missing asteroid. Dr. Luther had previously looked for the planet in vain in 1882, 1885, and in March of the present year.

The Ellipticity of Uranus.—Prof. W. Valentiner, of the Karlsruhe Observatory, writing in the *Astronomische Nachrichten*, No. 2781, states that Herr von Rebeur, observing Uranus with the 6-inch refractor of the Observatory on the evening of April 3, noticed that the disk appeared distinctly elliptical. On the following evening Prof. Valentiner himself observed the planet, and found that the direction as estimated of the major axis of the disk, the ellipticity of which was unmistakable, agreed closely with that of the assumed equatorial diameter as read off from the position-circle of the micrometer. Prof. Valentiner brings the subject forward now in the hope that those astronomers who have the command of powerful telescopes may be induced to give some attention to it.

The Washington Observatory.—It appears from *Science*, vol. ix. No. 218, that the statement (reported in last week's Nature, p. 595) that the sum of 400,000 dollars had been appropriated by Congress for the erection of a new Naval Observatory near Washington is erroneous. The amount actually available is but 100,000 dollars, with the understanding that the entire cost of the work shall not exceed 400,000 dollars.

The Paris Conference.—The same number of *Science* states that Ensign Winterhalter, of the United States Naval Observatory, had sailed for Paris, to represent the Observatory at the Photographic Conference. It is understood that Dr. Peters, of Clinton, and Mr. Rutherfurd, of New York, will also attend the Conference.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 MAY 1–7

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 1

Sun rises, 4h. 34m.; souths, 11h. 57m. 0·2s.; sets, 19h. 20m.; decl. on meridian, 15° 4′ N.: Sidereal Time at Sunset, 9h. 58m.

Moon (Full on May 7) rises, 11h. 27m.; souths, 18h. 59m.; sets, 2h. 19m.*; decl. on meridian, 14° 0′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	4 6	10 30	16 54	3 59 N.
Venus	5 58	14 21	22 44	24 16 N.
Mars	4 32	11 51	19 10	14 24 N.
Jupiter	18 1	23 14	4 27*	9 59 S.
Saturn	8 30	16 38	0 46*	22 21 N.

* Indicates that the setting is that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

May	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image °	 °
1	o′ Cancri	6	0 25	0 53	172	236
5	γ Virginis	2½	0 25	1 8	141	230
5	B.A.C. 4277	6	1 40	1 46	186	199
5	l^1 Virginis	5	21 30	near approach	323	—

May	h.	
1	15	Venus at least distance from the Sun.
6	8	Jupiter in conjunction with and 3° 14′ south of the Moon.

Saturn, May 1.—Outer major axis of outer ring = 39″·9; outer minor axis of outer ring = 16″·4; southern surface visible.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	May 4,	3 40	*m*
S Aurigæ	5 19·7	34 3 N.	,, 3,		*M*
R Canis Minoris	7 2·5	10 12 N.	,, 6,		*m*
R Camelopardalis	14 26·2	84 21 N.	,, 4,		*M*
δ Libræ	14 54·9	8 4 S.	,, 4,	20 11	*m*
U Coronæ	15 13·6	32 4 N.	,, 7,	22 7	*m*
S Herculis	16 46·8	15 8 N.	,, 3,		*m*
R Ophiuchi	17 1·3	15 57 S.	,, 2,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 4,	2 37	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·8	29 35 S.	May 2,	22 0	*m*
			,, 6,	2 0	*M*
T Herculis	18 4·8	31 0 N.	,, 7,		*m*
U Sagittarii	18 25·2	19 12 S.	,, 2,	2 0	*m*
			,, 5,	1 0	*M*
β Lyræ	18 45·9	33 14 N.	,, 2,	22 0	*M*
			,, 6,	4 0	m_2
R Aquilæ	19 0·9	8 4 N.	,, 2,		*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers

	R.A.	Decl.
Near υ Herculis	236°	47° N.
η Ophiuchi	254	21 S.
ζ Draconis	262	64 N.
α Aquilæ	298	5 N.

GEOGRAPHICAL NOTES

WE are glad to know that General R. Strachey, R.E., F.R.S., has agreed to accept the presidency of the Royal Geographical Society, in succession to Lord Aberdare. The Society's honours for the present year will be conferred as follows:—The Founder's Medal to Lieut.-Colonel T. H. Holdich, R.E., for the eminent services he has rendered to geography in Afghanistan; the Patron's Medal to the Rev. George Grenfell, for the extensive explorations he has carried out during his thirteen years' residence in Africa, partly in the Cameroons country and more recently in the Congo region; the Murchison Grant to Mr. George Bourne, second in command and sole survivor of the Landsborough Expedition, which crossed the continent of Australia in 1861; the Back Premium to Sarat Chandra Das, for his researches in Tibet; the Gill Memorial to Mr. J. F. Needham, for his explorations in the Lohit Valley of the Brahmaputra. The following have been made Honorary Corresponding Members:—H.R.H. Krom Mun Damrong Rajah Nubharp, Director-General of Surveys and Minister of Public Instruction, Siam; Dr. Alfred Kirchhoff, Professor of Geography at Hallé University, and President of the Hallé Geographical Society; and Dr. E. Naumann, late Director of the Geographical and Topographical Survey of Japan.

THE paper read at the Royal Geographical Society on Monday was by General J. T. Walker, F.R.S., on the Lu River of Tibet, the Lu-Kiang, or Lu-tse-Kiang of the Chinese. This river is generally held to be the source of the Salwin, but General Walker adduced many reasons for maintaining that it is more probably that of the Irrawadi. In the course of an able paper, the result of much research, General Walker gave a most useful summary of exploration in this highly interesting hydrographical region; more particularly insisting on the value of the work of the late Abbé Krick, who ascended the Lohit in 1852, but of whom little is known in this country. General Walker made out a strong case for his position, but the leading conclusion of his valuable paper is that further exploration in this remarkable region is urgently demanded. Probably no region of the earth would yield more valuable results to scientific geography.

ACCORDING to the latest news, Mr. Stanley is well up to time in his ascent of the Congo with the Emin Pasha Expedition. He is at present on his march across country from Matadi, at the lower end of the Livingstone Falls, to Leopoldville, on Stanley Pool. It is hoped that when he reaches the Pool he will find sufficient vessels in readiness to convey his large following up the river without delay. So far the Expedition has been exceedingly fortunate.

THE statement that Baron Nordenskjöld will undertake an Antarctic expedition at the expense of the King of Sweden and Mr. Oscar Dickson is, to say the least, premature. We are informed by Mr. Dickson that Baron Nordenskjöld is "willing" to undertake such an expedition, but that if he does so neither the King of Sweden nor Mr. Dickson will find the money. No doubt Baron Nordenskjöld would be an excellent leader for such an expedition, and as Committees have been formed both in this country and in Australia to promote Antarctic exploration, would it not be wise in them to unite their forces, and place themselves in communication with the Baron? Those who are competent to give an opinion on the subject maintain that an Antarctic expedition is much less risky than one to the other Pole. There would be no difficulty in a party wintering on some part of the Antarctic continent; a vessel could cruise round the verge of the ice during the winter and watch a favourable opening, of which immediate notice could be given to the exploring party, while a third vessel could leave New Zealand at a suitable time with additional supplies. No doubt the subject will again be brought up at the next meeting of the British Association, when it is hoped a strong and active Committee will be appointed. Baron Nordenskjöld will be among the distinguished foreigners invited to the meeting, and we hope he will accept the invitation.

THE Germans continue to show great activity in the exploration of their portion of New Guinea. Freiherr von Schleinitz has recently accomplished a running survey of Huon Gulf, and besides establishing the direction of the coast-line and the positions of reefs, has laid down eight hitherto unknown harbours and discovered nine new rivers. Some of them, especially the Markham River, would form excellent routes for the exploration of the interior; the broad valley of the latter extends for miles between high ranges of mountains. The south coast of Huon Gulf consists exclusively of primitive and metamorphic rocks, with older sedimentary rocks and volcanic formations. At a later date a further survey was made of the coast from Astrolabe Bay to the mouth of the Empress Augusta River, and led to the discovery of a series of bays, harbours, islands, and rivers.

M. GRIMAILO, in company with his brother, an engineer, and six Cossacks, has set out for a further exploration of the Pamir.

DR. LABONNE left Cherbourg a few days ago for a further exploration of the geysers and glaciers of Iceland.

PRELIMINARY NOTE ON THE FOSSIL REMAINS OF A CHELONIAN REPTILE, CERATOCHELYS STHENURUS, FROM LORD HOWE'S ISLAND, AUSTRALIA [1]

THE interesting remains of which I propose to give a brief notice in the present communication are contained in a friable sandstone (apparently formed of concreted blown sand), and they have a very recent appearance. The age of the deposit in which they are found is unknown, but it is probably Quaternary. The specimens have been for some years in the palæontological collection of the British Museum, and, for the most part, they have not yet been submitted to careful examination. But I learn that the greater number of them were long since rightly determined to be Chelonian by Mr. Davis, and set aside as such.

Several of the most important of these numerous and, in general, very fragmentary bones were originally found imbedded close together in the same block of sandstone. They consist of a great part of a pelvis, a caudal vertebra, and an imperfect skull. Of the pelvis, a right ischium and a pubis are imbedded in the rock, while an imperfect right ilium, which fits well on to the ischium, is separate; all these bones are unmistakably Chelonian. The caudal vertebra has remarkable peculiarities. It resembles an ordinary Chelonian caudal vertebra from the anterior half of the tail, in its general characters; but it is strongly opisthocœlous, the centrum having a deep cup behind and a correspondingly curved articular head in front. From the posterior part of the ventral face two stout processes diverge, and present terminal rounded facets for the rami of the large chevron bone which must have articulated with them. As a general rule, the caudal vertebræ of Chelonia are procœlous—but *Chelydra* and *Gypochelys* (perhaps also *Staurotypus* and *Platysternum*) form well-known exceptions,[2] in so far as the vertebræ behind the third and fourth are strongly opisthocœlous. In fact, the vertebra in question closely resembles the sixth or seventh of *Chelydra* or of *Gypochelys* (see Figs. 1 and 2). In the first, however, the transverse processes are very much stronger, and the pentagonal platform into which the upper surface of the neural arch expands, in place of a neural spine, is as long as the vertebra, instead of being only about half as long. The stout pre-zygapophysis of the right side is broken off, leaving only the base visible in the fossil.

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[1] Paper read at the Royal Society, by Prof. Thomas H. Huxley, F.R.S., on March 31.

[2] The opisthocœlous character of most of the caudal vertebræ of *Chelydra* was first pointed out by Von Meyer in his description of the Œningen *Chelydræ*. Baur ("Osteologische Notizen," *Zool. Anzeiger*, No. 238, 1886) has gone fully into the question, and has pointed out the exceptional nature of their structure among the Chelonia. Since the above paragraph was written, Dr. Günther has kindly enabled me to examine a spirit specimen and a skeleton of *Platysternum*. The caudral vertebræ resemble those of *Chelydra*, except that the last nine are procœlous, while that between these and the more anterior opisthocœlous vertebræ is nearly flat at the ends. In this, as in other respects, *Platysternum* presents characters intermediate between *Chelydra* and the ordinary *Emydæ*. Prof. Cope ("Vertebrata of the Tertiary Formations of the West," 1883, p. 111) ascribes opisthocœlous caudal verebræ to the *Bænidæ*, but no figures or descriptions of such vertebræ are given. Of the opisthocœlous Chelonian vertebræ figured in Plate XXIV. of the "Report of Extinct Vertebrata obtained in New Mexico" (1887), it is expressly stated that their "correct reference cannot now be made" (p. 43).

Two other caudal vertebræ, having the same structural features, occur among the detached remains, and belong, like the first, to the second fourth of the tail. Another tolerably complete vertebra, with a considerably longer centrum, corresponds very closely with a caudal vertebra of *Gypochelys* from the third fourth of the tail. In this, as in one of the foregoing vertebræ, the chevron bones are anchylosed with the centrum. I conceive, then, that there can be no doubt that the pelvic bones and these caudal vertebræ belonged to a Chelydroid Chelonian, of about the size of the largest "snapping turtles" which are met with in North America at the present day.

Primâ facie, the skull found in the same block might also be expected to be that of a Chelydroid; and, in fact, it is so. I do not base this interpretation on the Chelonian character of the upper jaw, as there are various extinct Saurian reptiles which closely approximate to Chelonia in this part of their structure. The diagnostic characters lie in the back part of the skull; and especially in the auditory region, which is altogether Chelonian. Not only so, but when this fragmentary skull is compared with that of *Chelydra*, the correspondence between the two is singularly exact (Figs. 3 and 4). In two respects, however, the fossil differs from *Chelydra* and *Gypochelys*.

(1) The roof over the temporal fossa formed by the parietal, post-frontal, and other bones, which leaves the auditory region uncovered in the recent genera,[1] extends back, beyond the occiput, in the fossil, and sends down a broad vertical rim from its margin.

(2) The upper surface of the cranial shield is, at most, rugose in the recent *Chelydridæ*; in the fossil, three strong conical processes, like horn-cores, of which the middle is the longest, are developed from its posterior and lateral region.[1]

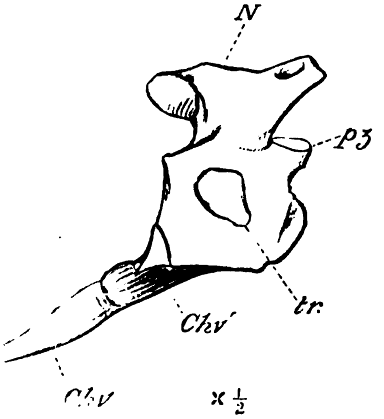

FIG. 1.—Caudal vertebra of *Ceratochelys*. *N*, platform on the neural arch; *pz*, pre-zygapophysis mutilated; *tr*. broken transverse process; *Chv'*, processes for the chevron bone; *Chv*, chevron bone.

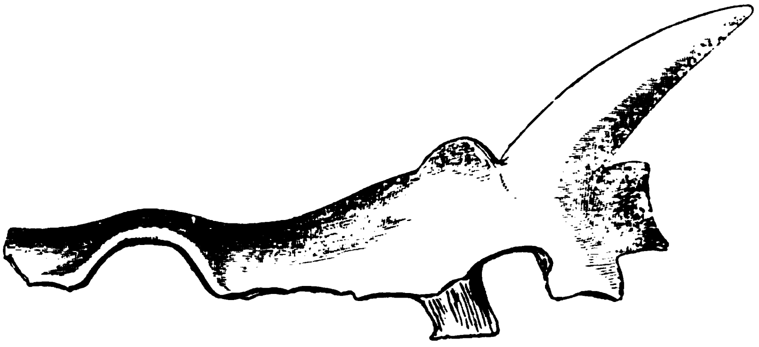

FIG. 3.

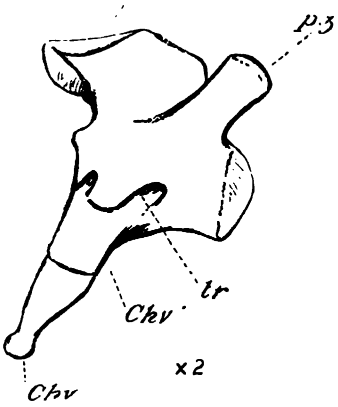

FIG. 2.—Caudal vertebra of *Chelydra*. Letters as in Fig. 1.

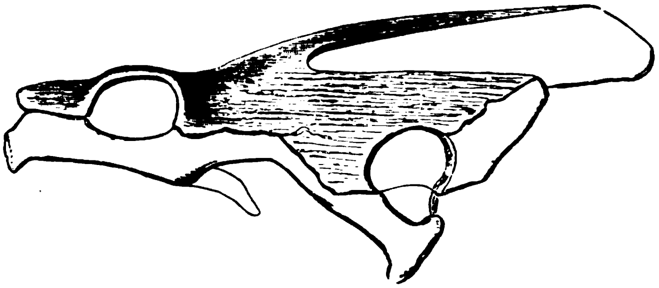

FIG. 4.

FIGS. 3, 4.—Skulls of *Ceratochelys* (Fig. 3) and *Chelydra* (Fig. 4); the latter of the natural size, the former much reduced. The portion of the skull of *Chelydra* which corresponds with the fossil is shaded.

This skull is described and figured in the Philosophical Transactions for 1886 (Plate 30, Fig. 1) by Sir R. Owen, under the generic or sub-generic name of *Meiolania*, and is said to belong to a Saurian reptile closely allied to the "*Megalania prisca*" described in earlier communications. But the skull is assuredly that of the Chelydroid Chelonian to which the pelvis and caudal vertebra belong. What *Megalania prisca* may be I do not pretend to say; but the remains which I have described can have nothing to do with any Saurian reptiles; and I propose to confer on the genus of Chelonia to which they belong the name of *Ceratochelys*.

The singular osseous caudal sheaths described by Sir R. Owen, in the same memoir, also appertain to *Ceratochelys*. They formed part of the series of remains sent to the British Museum along with the foregoing, in which none but Chelonian bones have yet been discovered; and the remains of vertebræ left in these sheaths are similar to the caudal vertebræ of the terminal fourth of the tail in the *Chelydridæ*. The snapping turtles are noted for the length and strength of the tail and for the strong, laterally-compressed, acuminated "scales" which form a crest along the median dorsal line, while others, less strongly keeled, lie at the sides of the tail. In many Chelonia, the extremity of the tail is enveloped in a continuous sheath. These and other scale-like structures in the Chelonia are usually spoken of as if they were entirely epidermal. But, a day or two ago, Dr. Günther informed me that in the Australian tortoise, *Manouria*, the great imbricated scales of the limbs contain bony scutes; and that similar scutes are to be found in *Testudo græca*. This, of course, suggested the examination of the caudal scales of *Chelydra* and *Gypochelys*; and, having been enabled by Dr. Günther's kindness to examine the caudal scales of a good-sized specimen of the latter, I have found that those of the crest contain bony scutes.[1] The bony scute corresponds very closely in form with the whole "scale," but the recurved apex of the latter is formed only by epidermal substance (Figs. 5 and 6).

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[1] The "roof" extends much further back in *Platysternum*.

[1] It is possible that these may be dermal bones coherent with the proper cranial shield.

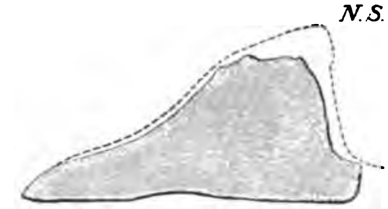

Fig. 5.

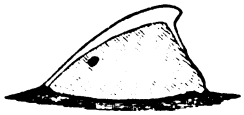

Fig. 6.

Figs. 5, 6.—Sectional views of a scute of the tail-armour of *Ceratochelys* (Fig. 5), and of one of the crest plates of *Gypochelys*, both of the natural size.

The living *Chelydra*, therefore, has a caudal armature which, in principle, is similar to that of *Ceratochelys*, but the osseous elements are relatively atrophied. There is exactly the same relation between the armour of species of living *Crocodiles* and *Alligators*, on the one hand, and those of *Jacare* and *Caiman* and the extinct *Teleosauria*, on the other. In the former, the epidermal scales remain well developed on the ventral side of the body, while the corresponding osseous scutes, fully developed in *Jacare*, *Caiman*, and *Teleosauria*, have vanished.

Among the detached fragments to which I have referred, there are remains of ribs, with their costal plates; marginal and other plates of the carapace; parts of the plastron; part of a scapula; sundry limb bones; and several of the cranial processes called "horn-cores." They all agree, so far as they can be compared, with the determination already arrived at; which, to sum it up in a few words, is that the remains of crania and caudal sheaths from Australia, hitherto referred to Saurian reptiles, under the names of *Megalania* and *Meiolania*, appertain to a hitherto unknown species of Chelonian, *Ceratochelys sthenurus*, closely allied to the living *Chelydra*, *Gypochelys*, and *Platysternum*.

The evidence of this fact offered in the present note appears to me to be conclusive, but it may be desirable hereafter to figure the parts mentioned and to describe them at length.

The interest which attaches to the discovery of this singular Chelonian arises partly from the fact that the group of Chelonia to which it belongs is wholly unrepresented in the fauna of Australia, as at present known. *Platysternum* is usually said to be found in China. Dr. Günther, however, informs me that Upper Burmah is its proper habitat; otherwise, North America, east of the Rocky Mountains, is the nearest region in which the *Chelydridæ* are to be found. But *Chelydridæ*, and, indeed, species of the genus *Chelydra*, occur in Upper Miocene (Œningen) and in Eocene formations in Europe. Moreover, *Platychelys*, of the Upper Jurassic series of Bavaria and Switzerland, is regarded by Rütimeyer as an early form of the group.

Lord Howe's Island is about 200 miles from the nearest Australian mainland, and something like 400 miles, as the crow flies, from the Darling Downs, in which the caudal armour, which has been ascribed to *Megalania*, was found. The discovery of *Ceretochelys*, therefore, has an interesting bearing on the question of the former extension of Australia to the eastward, on the one hand; and of the possible derivation of such forms as *Ceratochelys* from Asia, on the other hand. An elevation of the sea-bottom of 6000 feet would place Norfolk Island and Lord Howe's Island on a peninsula extending from the region of the present Barrier Reef to New Zealand; and the floræ and faunæ of those islands are known to have special affinities with those of New Zealand, and none with those of Australia.

Speculations respecting the origin of the Chelonian carapace are suggested by the discovery of osseous scutes in the vertebral region of the tail, and their coalescence in *Ceratochelys* to form a sort of caudal carapace, ridged in a manner resembling that of *Chelydra* and *Platychelys*. But the consideration of these points would take me beyond the limits of the present note.

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[1] The fact is noted by Rütimeyer (Lang and Rütimeyer, "Die Fossilen Schildkröten von Solothurn," *Denkschriften der Allg. Schweiz. Gesellschaft*, vol. xxii.). The armature of the tail in *Platysternum* is for the most part arranged in zones, of four plates in each zone; but I have not yet been able to find any bone in them.

THE WORK OF THE IMPERIAL INSTITUTE[1]

I.

THE Colonial and Indian Exhibition, which owes not only its conception, but also its brilliantly successful realisation to your Royal Highness, will be pre-eminently remarkable in times to come, for having achieved many results of vital importance and highest benefit to Her Majesty's subjects in all parts of her vast realms.

The collection of all that is commercially valuable and scientifically interesting of the natural products of the great Indian Empire and of the Colonies in one Exhibition, embracing as it also did very comprehensive illustrations of the development of commerce, of the arts, and of certain industries, in the many countries beyond the seas which combine with the United Kingdom to constitute our vast Empire, afforded those at home an opportunity, surpassing all previous conception, of studying and comparing the natural history and resources of those distant lands, of which, attached though we might be individually to one or more of them by ties of friendship or of interest, the knowledge of many of us was of a very vague or partial character.

To the Colonists who visited us last year, the Exhibition has been of inestimable value, in affording them a most favourable and appropriate opportunity of becoming acquainted or renewing their old friendship with the mother country, and of examining the progress there made in industrial, educational, and commercial development; in leading to the cultivation of intimacy between Colonists from different sections of the Queen's Dominions; and in affording them invaluable opportunities of comparing the resources and state of development of their respective countries with those of other parts of Europe. No more convincing illustrations than were provided by this great Exhibition could have been conceived of the importance to the home country, to each Colony, and to India, of fostering intimate relationship and unity of action. No more encouraging proof could have been afforded of the desire of all classes of Her Majesty's subjects at home to cultivate a knowledge of those far-off countries which the enterprise and perseverance of the British, and men of British offspring, have converted into prosperous and important dominions, chiefly during the period of the Queen's reign, than was furnished by the interest which the thousands upon thousands, who came from all parts, displayed in the study of the instructive collections in the galleries at South Kensington.

It was the success of the Exhibition which led to the definite formulation of the suggestion first made by your Royal Highness in a letter addressed by you in the autumn of 1884 to the Agents-General of the Colonial Governments, that a permanent representation of the resources of the Colonies and India, and of their continually progressing development, might, with great benefit to the Empire at large, be established in this country. That the realisation of this idea upon a sufficiently comprehensive basis might constitute a worthy memorial of the accomplishment of fifty years of a wise and prosperous reign; a memorial not personal in its character excepting so far as it constituted an emblem of the love and loyalty of Her Majesty's subjects, but tending, as she would most desire, to serve the interests of the entire Empire; this had only to be pointed out by your Royal Highness to be heartily concurred in by the official representatives of the Colonies and India, who were so intimately identified with the triumphs of the recent Exhibition.

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[1] Lecture (abridged) delivered at the Royal Institution, on Friday, April 22, by Sir Frederick Abel, C.B., F.R.S.; H.R.H. the Prince of Wales, K.G., F.R.S., Vice-Patron, in the Chair.

The Committee to whom you, Sir, intrusted the elaboration of a scheme for carrying this conception into effect, became persuaded by a careful consideration of the subject that such an Institution as your Royal Highness desired to see spring into life, to be a memorial really worthy of the Jubilee of Her Majesty's reign, and to fulfil the great purposes which you had in view, must not be confined in its objects to particular portions of Her Majesty's Dominions, but must be made thoroughly representative of the interests and of the unity of the whole Empire.

The outline of the scheme for the establishment of an Imperial Institute for the United Kingdom, the Colonies, and India, which met with the cordial approval of your Royal Highness, was necessarily concise in dealing with the very wide extent of ground which the operations of the Institute are intended to cover; but those who have carefully considered it, and rightly interpreted its proposals, have not failed to realise that it aims at very much more than the creation and maintenance of collections illustrative of the natural resources of our Colonies and of India, and of the development and present condition of the chief industries of different parts of the Empire.

One of the primary objects of the Institute will certainly be the establishment of thoroughly well selected, carefully arranged, and efficiently maintained representations of the natural products which constitute the treasures, and are emblematic of the important positions in the Empire, of those great colonial possessions which, during the fifty years of Her Majesty's reign, have, in many instances, experienced a marvellous development in extent, in commercial, social, and even in political importance.[1] The recent Exhibition not only afforded conclusive demonstration of the great interest and value to the United Kingdom which must attach to such collections if properly organised; it also served, by such illustrations as the magnificent collections of valuable woods, from nearly every colony, many quite unknown in England, and the great variety of valuable economic products from India, of the existence of which we at home had little idea, to convince us that our knowledge of the great countries which constitute the chief portion of the Empire is very limited and imperfect, and that their resources are in many directions still in the infancy of development. Our Colonial brethren cannot, on their part, fail to be greatly benefited by being thoroughly represented in a well-selected and carefully organised assemblage of illustrations of the sources of prosperity which constitute the sinews of their commerce, and upon a continued exploration and cultivation of which must depend the maintenance of their influence upon industrial and social progress. Neither can they fail to reap substantial advantages by pursuing a friendly rivalry with each other in demonstrating the advances made from time to time in the development of the resources of the respective portions of the Empire in which their lot is cast.

The hearty co-operation and important material support to which the great Colonies, through their representatives in London, pledged themselves when the scheme for the proposed Imperial Institute was in the first instance limited to this branch of the great work which it is now contemplated to accomplish, afforded conclusive evidence of their earnest desire to be in all respects thoroughly represented in the mother country, and to take their places permanently in our midst as fellow-labourers in the advancement of the prosperity of the Empire. In furtherance of this important end, a notable feature of that building which, in its character, will, it is hoped, be worthy of the momentous epoch which it is to commemorate, will be the attractions and conveniences presented by it, as a place of resort and a *rendezvous* for Colonists visiting England, and, it is also anticipated, for the important Societies which represent the Colonies and Asiatic possessions in this country, and the facilities which it will afford for reference to literature concerning the Colonies and India, for conferences on matters of common interest and value to the Colonists and those at home, for the interchange of information between the British manufacturer and those in the Colonies who are directly interested in meeting his requirements, and generally for the cultivation of intimate relations and good fellowship between ourselves and our brethren from all parts of the Empire.

The Institute will, however, not only operate actively under its own roof in promoting the cultivation of a better knowledge of the geography, natural history, and resources of our Colonies, and for the advancement of the interests of the Colonists in this country; it is also contemplated that representative collections of the natural products of the Colonies and India, carefully identified with the more elaborate collections of the head establishment, shall be distributed to provincial centres, and that the provinces shall be kept thoroughly conversant with the current information from the Colonies and India, bearing upon the interests of the commercial man, the manufacturer, and the intending emigrant.

Although the formation and maintenance up to date of collections illustrative of the development and present condition of the important industries of the Empire also forms, as I have stated, a part of the programme of the Institute, the scope of its activity in relation to industry will be of a much more comprehensive character; indeed, it is to be hoped that the work which it will achieve in furtherance of the development and progress of industries and their future maintenance in the United Kingdom at least upon a footing of equality with their conditions in the great Continental States, will be most prominent in securing to the Imperial Institute the exalted position which it should occupy as the National Jubilee Memorial of Her Majesty's reign.

There is no need for me to recall to the minds of an audience in the Royal Institution the great strides which have been made during the last fifty years in the applications of science to the purposes of daily life, to the advancement of commerce, and to the development of the arts and manufactures. Nor is it necessary to dwell upon the fact that this country is the birth-place of the majority of the great scientific and practical achievements which have revolutionised means of intercommunication, and have in other ways transformed the conditions under which manufactures, the arts, and commerce are pursued. These very achievements, of which we as a nation are so justly proud, have led, however, by many of their results, to our becoming reduced to an equality of position with other prominent nations in regard to important advantages we so long derived from the possession in this country of great material resources easy of access and application, and from the consequent pre-eminence in certain branches of the trade and industry which we so long enjoyed.

In 1852, Sir Lyon Playfair was impelled by the teaching of the preceding year's Great Exhibition to point out that "the raw material, formerly our capital advantage, was gradually being equalised in price and made available to all by the improvements in locomotion," and "that industry must in future be supported, not by a competition of local advantages, but by a competition of intellect." If this was already felt to be the state of the case six-and-thirty years ago, how much more must we be convinced of the full truth of this at the present day, by the conditions under which the British merchant and the manufacturer have to compete with their rivals on the Continent and in the United States.

It is still within the recollection of many that almost the whole world was in very great measure dependent upon Great Britain for its supplies of ordinary cast iron. Even as lately as 1871, the United States of America received from Great Britain nearly one-fifth of its total produce of pig iron; but from 1875 all importation of British iron ceased for over three years, and it was only in consequence of requirements in the States exceeding the capabilities of production, that some small demands arose in 1879, which were for some time maintained.

Within three years, however, the make of iron in the United States increased by 70 per cent., and although, since 1882, the actual make has not increased, the *capacity of production* has risen enormously, it being at present estimated at nearly 300 per cent. greater than it was in 1879. Looking nearer home, we find that the iron of France, Belgium, and Germany not only competes with ours in the open market, but that Belgian and German iron is actually imported into this country to a moderate extent.

From time to time the ground which we have lost through the development of the resources of other countries has been more than retrieved temporarily by improvements effected through the more thorough comprehension and consequent better application of the scientific principles underlying processes of manufacture, but the ultimate effect of advances of importance has not unfrequently been to improve the position, in relation to the manufactures concerned, of other nations less favourably circumstanced than Great Britain.

The history of the development of steel manufacture during the last twenty-five years affords a most instructive illustration of the fluctuations which may ensue in the value of our natural resources, and the consequent condition of one or other of our important industries, arising out of continued advances made in the application of science to the perfection or transformation of manufacturing processes, and of the stimulating effects of such fluctuations upon the exertions of those who are able to bring scientific knowledge to bear upon the solution of problems in industrial operations which entirely baffle the ordinary manufacturer. Within that period the inventions of Bessemer and of Siemens have led to the replacement of iron by steel in some of its most extensive applications. This important change in our national industry was, ere long, productive of a serious crisis therein, and for the reason that the pig iron produced from a large proportion of those ores which, from their abundance and the cheapness of their treatment, have been greatly instrumental in placing Great Britain in her high position as an iron-producing nation, could not be applied to the production of marketable steel by means of the Bessemer converter. Hence the application of this rapid steel-making process had to be chiefly restricted to particular kinds of ores, free from the impurity, phosphorus, which it was powerless to eliminate; the supplies of such ores being limited to a few districts in this country. These had to be largely supplemented by importations from other countries; nevertheless the cheapness of production and superiority in point of strength, durability, and lightness of the steel rails thus sent into the market from the Bessemer converter combined to maintain a supremacy of them over iron rails, &c., manufactured by the old puddling process from the staple ores of the country.

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[1] Statistical statements illustrating the development of the colonies during the Queen's reign are appended.

The advantages presented by steel over the wrought iron of the puddling furnace for constructive purposes speedily became evident, and the effect of the rapid displacement of malleable iron by steel produced from ores of a particular class has been that at least 85 to 90 per cent. of the iron ores of Great Britain could no longer be applied to the production of material for rails, for ships, and for other important structures. Great has been the apprehension among the owners of those ores that the demand for iron which they can furnish could not revive, but the scientific metallurgist has successfully grappled, from more than one direction, with the great problem of restoring their commercial importance; and a simple alteration of the method of carrying out the Bessemer process has within the last few years led to really triumphant results, with the employment of those ores which before could only be dealt with by the searching operation of the old puddling furnace. A new era has thus been established in steel manufacture, there being now but very few restrictions to the application of the quick processes to iron produced from all varieties of ores. Indeed, the treatment is actually being applied profitably to the recovery of iron from the rich slag forming the refuse-product of the puddling furnace in the production of malleable iron, which before had been condemned to limited usefulness as a material for road-making. Yet another most interesting and valuable result has been achieved by this simple application of scientific knowledge. The slag or refuse-product of the so-called *basic treatment* of iron contains, in the form of phosphates of lime and magnesia, the whole of the phosphorus which it is the main function of that treatment to separate from the metal; it was soon found that the phosphoric acid was there presented in a condition as readily susceptible of assimilation by plants as it is in the valuable artificial manure known as superphosphate; this refuse-slag, simply ground up, constitutes therefore a valuable manure which already commands a ready sale at very profitable prices.

The origination of this latest advance in the development of steel manufacture dates back only nine years, and already the year's product of the basic process amounts to over 1,300,000 tons of steel. But although it is to Englishmen that the owner of iron property and the steel-maker are again indebted for these important results, and to English manufacturers that the first practical demonstration of the success of this process is due, its application has been far more rapidly elaborated upon the Continent than here; in Germany the importance of the subject was at once realised, and it is there that considerably the largest proportion of steel is produced by the basic treatment; it is in Germany also that the value of the slag for agricultural purposes has been developed, the first steps in its utilisation here being but just now taken, in Staffordshire.

I have already referred to the remarkable strides which have been made in the extension of iron manufacture in the United States; the development there of steel production has been no less marvellous, and the causes of this are evident; the resources of the country in ore and fuel are gigantic, and the systematic technical training of the people has made its influence felt upon the development of this as of every other branch of industry which our friendly rivals pursue. But it is not only in the United States that the development in the production of iron and steel has greatly increased of late years; thus, in Germany the increase in the production of pig iron alone, during the last twenty-one years, has been 237 per cent., while with us it has been 75 per cent.

Although, however, the increase in actual production of iron and steel in Great Britain has not kept pace with that of some other countries, it is satisfactory to know that our *productive power* has very greatly increased in late years, and there is probably no one branch of our industries in which we have maintained our position so satisfactorily in regard to quality of product as that of iron and steel manufacture, even though, every now and then, we have indications that in the struggle with other nations for superiority of product and for pre-eminence in continuity of progress, we have to look to our laurels.

There are, however, other important branches of industry, for a time essentially our own, the present condition of which, in this country, we cannot contemplate with equal satisfaction. Several instructive illustrations might be quoted, but I will content myself with a brief examination of one of the most interesting.

In illustrating the advances which were being made, thirty-five years ago, as demonstrated by the Exhibition of 1851, Playfair referred to the great development of the value of the evil-smelling coal-tar, which was then made to furnish the solvent liquids benzene and naphtha, and the antiseptic creosote, the residual material being utilised for pavements and for artificial fuel. The chemist little dreamt then that between 1851 and the year of the next great Exhibition, 1862, coal-tar would have become a mine of wealth equally to science, the manufactures, and to the arts, in which fresh workings have ever since continued to be opened up, and still present themselves for exploration. Hofmann, in his valuable report on the chemical products and processes elucidated by that Exhibition, dwells with the enthusiasm of the ardent worker in science upon the brilliant products obtained from coal-tar which had resulted from the labours of the scientific chemist, and had already acquired an almost national importance, although this great industry was then still in its infancy. From the year 1856, when the first colouring-matter, known as *mauve*, was discovered and manufactured by one of Hofmann's most promising young pupils, Mr. Perkin, down to the present time, the production of new coal-tar colours, or of new processes for preparing the known colours in greater purity, has progressed uninterruptedly, this industry having long since become one of the most important, and also one of the most remarkable, as illustrating by each stage of its development the direct application of scientific research to the attainment of momentous practical results.

The difficulties to be overcome before mauve could be produced upon a manufacturing scale were very great, and were only solved by a steady pursuit of scientific research, side by side with practical experiments suggested by its results. Aniline—the parent of the first coal-tar colour, a liquid organic alkali—a most fertile source of interesting and important discoveries in organic chemistry, was produced with difficulty by various methods in very small quantities, so as to be almost a chemical curiosity at the time of the discovery of mauve. Among the substances from which it had been prepared was the volatile liquid known as *benzene*, first discovered in the laboratory of this Institution in 1825 by Faraday, in the liquid products condensed from oil gas, but afterwards obtained by Mansfield, in the College of Chemistry, from coal-tar naphtha. The conversion of benzene into aniline was accomplished as a manufacturing process after many difficulties by Perkin; and within a year after the discovery of mauve by him, it was in the hands of the silk dyer. Perkin's success led other chemists at once to pursue researches in the same direction, especially in France, where the next important coal-tar colour, *magenta* or fuchsine, was obtained, by M. Verguin, the successful manufacture of which in a pure state was, however, first accomplished by English chemists. In 1861 beautiful violet and blue colours were produced, again by French chemists, but were manufactured shortly afterwards in a pure state in England.

The six years succeeding those which formed the first period (1854–62) of existence of this industry were fruitful, not only of many beautiful new dyes, first produced in England, but also of important progress made here, as well as on the Continent, in the development of the manufacture, and of our knowledge of the constitution, of the parent colours.

In the next period of six years (1868–74) another great stride was made in the coal-tar colour industry, due to important scientific researches carried out by two German chemists, Graebe and Liebermann, which led them, in the first place, to obtain an insight into the true nature of the colouring-matter of one of the most important staple dye-stuffs, namely the madder-root. They found that this colouring-matter which chemists call *alizarine* was related to *anthracene*, one of the most important solid hydrocarbons formed in the distillation of coal, a discovery which was speedily followed by the artificial formation of the madder-dye, alizarine, from that constituent of coal-tar. At first, this achievement of Graebe and Liebermann was simply of high scientific interest; but Perkin, and Graebe, and Liebermann, were not long in discovering methods by which the conversion of anthracene into the madder-dye could be accomplished on a large scale, and the manufacture of alizarine was soon most actively pursued in this country, with very momentous results, as regards the market value of the madder root. The latter has long been most extensively cultivated in Holland, South Germany, France, Italy, Turkey, and India, the consumption of madder in Great Britain having attained to an annual value of as much as 1,000,000*l.* sterling. Playfair pointed out in 1852 that important improvements had lately been attained in the extraction of the red colour or alizarine from the madder-root, but those results, most valuable at the time of the first Great Exhibition, became insignificant when once the dye was artificially manufactured from anthracene; the price paid for madder in 1869 was from 5*d.* to 8*d.* per pound, but now the equivalent in artificial madder-dye, or alizarine, of one pound of the root, can be obtained for one-halfpenny.

With the discovery of artificial alizarine the truly scientific era of the coal-tar industry may be said to have commenced, most of the commercially valuable dye-products, obtained since that time, being the result of truly theoretical research by the logical pursuit of definite well-understood reactions. The wealth of discovery in this direction made during the last thirteen years is a most tempting subject to pursue; but I must content myself with mentioning that one of the results was the production of very permanent and brilliant scarlet and red dyes, the manufacture of which has greatly reduced the market value of cochineal; that the careful study of the original coal-tar colours led to their production in a state of great purity by new and beautifully simple scientific methods; and that even the well-known vegetable colouring-matter, *indigo*, one of the staple products of India, now ranks among the colours synthetically obtained by the systematic pursuit of scientific research, from compounds which trace their origin to coal-tar.

The rapid development of the industry has also not failed to exercise a very important beneficial influence upon other chemical manufactures; thus, the distillation of tar, which was a comparatively very crude process, when, at the period of the first Exhibition, benzene, naphtha, dead-oil, and pitch were the only products furnished by it, has become a really scientific operation, involving the employment of comparatively complicated but beautiful distilling apparatus for the separation of the numerous products which serve as raw materials for the many distinct families of dyes. Very strong sulphuric acid became an essential chemical agent to the alizarine manufacturer, and, as a consequence, the so-called anhydrous sulphuric acid, the remarkable crystalline body which was for many years prepared only in small quantities from green vitriol, is now made at a low price upon a very large scale by a beautifully simple process worked out in England. The alkali- and kindred chemical trades have been very greatly benefited by the large consumption of caustic soda, of chlorate of potash, and other materials used in the dye manufactures, and the application of constructive talent, combined with chemical knowledge, to the production of efficient apparatus for carrying out on a stupendous scale the scientific operations developed in the investigator's laboratory, has greatly contributed to the creation of a distinct profession, that of the chemical engineer.

One of the most beneficial results of the rapid development of the coal-tar colour industry has been its influence upon the ancient art of dyeing, which made but very slow advance until the provision of the host of brilliant, readily-applicable colours completely revolutionised both it and the art of calico-printing.

I venture to think that it will be interesting at this point to quote some words of prophecy included in Prof. Hofmann's important "Report on the Chemical Section of the Exhibition of 1862," and to inquire to what extent they have been verified. In commenting upon one of the features of greatest novelty in that world's show, the exhibition of the first dye-products derived from coal-tar, he says:—

"If coal be destined sooner or later to supersede, as the primary source of colour, all the costly dye woods hitherto consumed in the ornamentation of textile fabrics; if this singular chemical revolution, so far from being at all remote, is at this moment in the very act and process of gradual accomplishment; are we not on the eve of profound modifications in the commercial relations between the great colour-consuming and colour-producing regions of the globe? There is fair reason to believe it probable that, before the period of another decennial Exhibition shall arrive, England will have learnt to depend, for the materials of the colours she so largely employs, mainly, if not wholly, on her own fossil stores. Indeed, to the chemical mind it cannot be doubtful that in the coal beneath her feet lie waiting to be drawn forth, even as the statue lies waiting in the quarry, the fossil equivalents of the long series of costly dye-materials for which she has hitherto remained the tributary of foreign climes. Instead of disbursing her annual millions for these substances, England will, beyond question, at no distant day become herself the greatest colour-producing country in the world; nay, by the strangest of revolutions, she may ere long send her coal-derived blues to indigo-growing India, her tar-distilled crimson to cochineal-producing Mexico, and her fossil substitutes for quercitron and safflower to China, Japan, and the other countries whence these articles are now derived."

So far as concerns the displacement of madder, cochineal, quercitron, safflower, and other natural dye-materials from their positions of command in the markets of England and the world, Hofmann's predictions have been amply fulfilled, and it appeared in the earlier days of the coal-tar colour industry as though he would be an equally true prophet in regard to England becoming herself the greatest colour-producing country in the world. But, although Germany did little in the very early days of this industry, beyond producing a few of the known colours in a somewhat impure condition, many years did not elapse ere she not only was our equal in regard to quality of the dyes produced, but, moreover, had outstripped us in the quantities manufactured and in the additions made to the varieties of valuable dyes sent into the market. So far back as 1878 the value of the make of colours in England was less than one-fourth that of Germany, and even Switzerland, which, in competing with other countries industrially, is at great natural disadvantages, was not far behind us, ranking equal to France as producers. The superior position of Germany in reference to this industry may be in a measure ascribable to some defects in the operation of our Patent Laws and to questions of wages and conditions of labour; but the chief cause is to be found in the thorough realisation, by the German manufacturer, of his dependence for success and continual progress upon the active prosecution of scientific research, in the high training received by the chemists attached to the manufactories, and in the intimate association, in every direction, of systematic scientific investigation with technical work.

The young chemists whom the German manufacturer attracts to his works rank much higher than ours in the general scientific training which is essential to the successful cultivation of the habit of theoretical and experimental research, and in the consequent power of pursuing original investigations of a high order. Moreover, the research laboratory constitutes an integral part of the German factory, and the results of the work carried on by and under the eminent professors at the universities and technical colleges are closely followed and studied in their possible bearings upon the further development of the industry.

The importance attached to high and well-organised technical education in Germany is demonstrated not only by the munificent way in which the scientific branches of the universities and the technical colleges are established and maintained, but also by the continuity which exists between the different grades of education; a continuity, the lack of which in England was recently indicated by Prof. Huxley with great force.

The important part taken by the German universities in the training of young men for technical pursuits has often been dwelt upon as constituting a striking feature of contrast to our university systems. The national appreciation of the opportunities there presented for scientific training is demonstrated by the large number of students which are always working in the university laboratories, while the expenditure of 30,000*l.* upon the physical laboratory, and 35,000*l.* upon the chemical department, of the New University of Strasburg, serves to illustrate the unsparing hand with which the resources of the country are devoted to the provision of those educational facilities which are the very life-spring of the industrial progress whence those resources are derived.

In France, higher education had been allowed to sink to a low ebb after the provincial universities had been destroyed in the great Revolution, and the University of Paris had been constituted by the first Napoleon the sole seat of high education in the country. Before the late war, matters educational were in a condition very detrimental to the position of the country among nations. There was no lack of educational establishments, but the systems and sequence of instruction lacked organisation, but since the war, France has made great efforts to replace her educational resources upon a proper footing. The provincial colleges have been re-established at a cost of 3,280,000*l.*, and the organisation of industrial education has now been greatly developed, though still not on a footing of equality with that of Germany. Every large manufacturing centre has its educational establishment where technical instruction is provided, with special reference to local requirements; and, in order to render these colleges accessible to the best talent of France, more than 500 scholarships have been founded, at an annual cost of 30,000*l.* The Ecole Centrale des Arts et Manufactures, of Paris, still maintains the reputation as the great technical university of the country which it earned many years ago, and receives students from the provincial colleges, where they have passed through the essential training preliminary to the high technical education which that great institution provides.

Switzerland has often been quoted as a remarkable illustration of the benefits secured to a nation by the thoroughly organised education of its people. Far removed from the ocean, girt by mountains, poor in the mineral resources of industry, she yet has taken one of the highest positions among essentially industrial nations, and has gained victories over countries rich in the possession of the greatest natural advantages. Importing cotton from the United States, she has sent it back in manufactured forms, so as to undersell the products of the American mills. The trade of watch-making, once most important in this metropolis, passed almost entirely to Switzerland years ago; the old-established ribbon trade of Coventry has had practically to succumb before the skilled competition of Switzerland; and although she has no coal of her own, Switzerland is at least as successful as France in her appropriation of the coal-tar colour industry and her rivalry in rate of production with England, the place of its birth and development. Comparative cheapness of labour will not go very far to account for these great successes; they undoubtedly spring mainly from the thoroughly organised combination of scientific with practical education of which the entire people enjoys the inestimable benefit.

Holland furnishes another brilliant example of the success with which a nation brings the power of systematic technical education to bear in securing and maintaining industrial victories in the face of most formidable disadvantages, while the United States of America, so rich in natural resources, have long since realised the immensity of additional advantages to be gained over European nations in the war of industry by a wide diffusion and thorough organisation of technical education. So long as forty years ago the States already possessed several excellent educational institutions established upon the basis of the Continental polytechnic schools, but it was not until about fifteen years later that the country became thoroughly impressed with the great advances achieved by Germany in technical education, and that the subject was made a thoroughly national one. It is now just upon a quarter of a century ago since Congress ordained that each State should provide at least one college, having for its leading objects the diffusion of scientific instruction in its relations to the industry of the country, and decreed that public lands should be granted to the States and Territories providing such colleges. The combined effect of this State action, and of great private munificence, was a remarkably rapid development of scientific and technical education throughout the country; besides some fifty colleges, with eight or nine thousand students, which sprang out of the Land Grant Act for Industrial Education, there are now in the States about 400 other universities and colleges, in a large proportion of which efficient instruction in applied science is provided. To the useful work accomplished within a few years by these and many other highly important educational institutions, which have placed the acquisition of scientific knowledge within the reach of the very humblest, the enormous strides made by the United States in the development of home industries must unquestionably be in the main ascribed.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—There are only a few alterations and additions to the usual scientific lecture-list to report, as most of the courses are in continuation of those given last term. Prof. Sylvester is to lecture on the theory of numbers; in the Chemical Department, Mr. Wyndham Dunstan lectures on organic chemistry in relation to medicine and physiology; Prof. Prestwich is to have his usual summer geological excursions; Dr. Tylor continues his exposition of the development of arts, as illustrated in the Pitt-Rivers Museum.

The Sibthorpian Professorship of Rural Economy has been filled up by the re-election of Dr. Gilbert. The Radcliffe Travelling Fellowship has been awarded to Mr. W. Overend, B.A., of Balliol. The statutes of the University Commissioners, which limited the competition for most college scholarships to candidates under nineteen, seem to be having an unfortunate, though not unexpected, effect, as several colleges have lately found it impossible to award scientific scholarships, owing to the want of sufficiently qualified candidates.

There is a good deal of strong feeling in the University with regard to the approaching appointment of a Reader in Geography, and the action of the Delegates of the University Fund in transferring a Readership from History to Geography, just after the offer of the Royal Geographical Society for a similar purpose had been refused, is the subject of much unfavourable comment. There is no thought of opposition to the study of geography, even of scientific geography, but history lecturers not unnaturally complain that the only University appointment open to them should be abolished to make a post for a lecturer in another subject.

CAMBRIDGE.—Among the seven courses on chemistry being delivered this term are lectures on gas analysis and on aromatic bodies, by Dr. S. Ruhemann. Prof. Dewar and Dr. Ruhemann also superintend laboratory practice specially directed to research.

The course given by Mr. Lyon, the Superintendent of the Mechanical Workshops, this term is on machine construction.

Mr. Langley is giving a special course on the central nervous system, with demonstrations and practical work.

Prof. Macalister's lectures this term are on the history of human anatomy. It is to be hoped that he will publish them.

Mr. M. A. Fenton is lecturing on elementary comparative osteology: Dr. Gadow on the morphology of Mammalia, recent and extinct, and on the palæontology of the Vertebrata.

Dr. Vines has a course of advanced embryology of plants, and Mr. F. Darwin is giving advanced demonstrations in the physiology of plants.

In geology, Prof. Hughes is taking the geology of the neighbourhood of Cambridge, by lectures and field excursions; Mr. Marr, foreign stratigraphy; and Mr. Roberts, the Trilobites.

Prof. Roy has classes for general pathology, morbid anatomy, and practical morbid histology, bacteriology, &c.

The lectures mentioned above are only a selection of the more interesting courses. The lists from which the above are selected announce about seventy-five courses of lectures and practical work.

Candidates for the John Lucas Walker Studentship, the holder of which must devote himself or herself to original research in pathology, should send their applications to Prof. Roy, Trinity College, Cambridge, not later than May 31 next. The Studentship is of the annual value of 250*l.*, and is tenable under certain conditions for three years.

SCIENTIFIC SERIALS

The American Journal of Science, April.—Contributions to meteorology, twenty-second paper, by Elias Loomis. In this communication the author treats of areas of high pressure, their magnitude and direction of movement, and their relation to areas of low pressure. The latter subject is illustrated with a plate giving the isobars for December 15, 1882, in the northern hemisphere, and it is shown generally that the movement of areas of high pressure depends upon very different causes from that of areas of low pressure, which seem to be endowed with a power of locomotion residing within themselves. Areas of high pressure exhibit no such power, their movement seeming to depend entirely on external forces.—The faults of South-West Virginia, by John J. Stevenson. Here is given a summary of the information communicated by the author in several memoirs read before the American Philosophical Society (1880-84) on a reconnaissance made by him of the faulted region in Virginia from the Tennessee line to nearly twenty miles beyond New River, a total distance of 150 miles.—On Taconic rocks and stratigraphy, with a geological map of the Taconic regions, part 2, by James D. Dana. The general facts are here detailed which bear on the geographical distribution of the limestone and other rocks. A description is given of Williamstown, regarded as the birthplace of the Taconic system, and it is shown generally that the great quartzite formation, forming the foundation of the Palæozoic of the region, derived its material from Archæan formations of the vicinity and not from the fabled "Atlantis" as some geologists have supposed.—Irish esker drift, by G. H. Kinahan. It is pointed out that, in common with some other observers, Prof. Carvill Lewis confounds true esker drift and ridges of esker-like drift. His statement in the December number of the *American Journal of Science* that the Irish eskers appear to be adjuncts of the melting of the ice-sheets is shown to be impossible.—Physical characteristics of the northern and north-western lakes, by L. Y. Schermerhorn. The results are given of the lately completed surveys made by the United States of all the great lakes draining through the St. Lawrence to the Atlantic. The total water-area is 95,275, and the total area of the lacustrine basin over 270,000, square miles. The length of shore-line with connecting rivers is about 5400 miles, the extreme depth of Superior 1008 feet, or over 406 below sea-level, the mean annual rainfall of the whole basin 31 inches, the volume of water in the lakes about 6000 cubic miles, and the discharge of Ontario at St. Lawrence River 300,000 cubic feet per second.—Mineralogical notes from the laboratory of Hamilton College, by Albert H. Chester. Specimens are described and analysed of fuchsite, pink celestite, zinkenite, brochantite, pectolite, crystals of barite, scorodite, and bismuthite.—The topography and geology of the Cross Timbers and surrounding regions in Northern Texas, by Robert T. Hill. This wooded zone penetrating in two belts from Indian territory through the surrounding prairie southwards to 32° N. lat., is explained by the detritus of arenaceous strata which occupy well-defined horizons in the geological series, and which have been exposed by the denudation of the overlying strata.—American Jurassic mammals, by Prof. O. C. Marsh. In this paper are described the remains of several hundred individuals which have come to light since the appearance of the author's previous papers on the subject. Although fragmentary, the remains are usually well preserved, comprising the lower jaws, teeth *in situ*, various portions of the skull, vertebræ, and other parts of the skeleton. Placental as well as marsupial mammals occur in the oldest formations, whence the inference that the former do not derive from the latter evolutionally, as is supposed, but that both of these orders descend in independent lines from a common ancestor.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 24.—"On the Magnetisation of Iron in Strong Fields." By Prof. J. A. Ewing, University College, Dundee, and William Low.

The behaviour of iron and steel when subjected to very strong magnetising forces is a matter of considerable practical and very great theoretical interest, especially from its bearing on the molecular theory of magnetisation, which assigns an upper limit to the intensity of magnetism that a piece of iron can acquire, and even suggests that the metal may become diamagnetic under the influence of a sufficiently great force. All experiments hitherto made, by magnetising iron in the field of an electric solenoid, have shown that the intensity of magnetism, $\mathfrak{I}$, as well as the induction, $\mathfrak{B}$, is increasing with the highest values actually given to the magnetising force, $\mathfrak{H}$. It is scarcely practicable, however, to produce by the direct action of a magnetising solenoid, a field whose force exceeds a few hundreds of C.G.S. units.

In the space between the pole-pieces of a strong electro-magnet we have a field of force of much greater intensity than it is practicable to produce by the direct action of the electric current. This field is not well adapted for experiments whose object is to determine with precision the relation of magnetisation to magnetising force, on account of the distortion which it undergoes when the piece of iron to be magnetised is introduced into it. It is, however, well suited for experiments whose object is to determine how much magnetism the metal can be forced to take up.

In the authors' experiments, bobbins of Lowmoor and Swedish wrought-iron and cast-iron were magnetised by placing them between the pole-pieces of a large electro-magnet. The bobbins consisted of a short narrow central neck with conical ends. The magnetic induction in the neck was measured ballistically by means of an induction coil, consisting of a single layer of fine wire, wound on the neck; and the magnetic field in the air-space immediately contiguous to the neck was also measured by means of a second induction coil wound over the first, and of slightly greater diameter. This enabled the non-ferrous space under the inner induction coil to be corrected for, and also gave an approximation to the value of $\mathfrak{H}$, the magnetic force acting on the metal. The magnetic force varied up to about 11,000 C.G.S. units, and the highest induction observed (in a sample of Lowmoor wrought-iron) was 32,880 C.G.S. units. In some instances the magnetic induction was observed by withdrawing the bobbin; in others the bobbin was turned round suddenly so that its magnetism was reversed. The following results refer to Lowmoor wrought-iron and to cast-iron.

Lowmoor wrought-iron

Field round iron neck per sq. cm.		Magnetic induction in the metal		Current in field magnets, amperes		$\frac{\mathfrak{B}-\text{outside field}}{4\pi}$		$\frac{\mathfrak{B}}{\text{outside field}}$
3,630	...	24,700	...	1·98	...	1680	...	6·80
6,680	...	27,610	...	4·04	...	1670	...	4·13
7,800	...	28,870	...	5·81	...	1680	...	3·70
8,810	...	29,350	...	7·60	...	1630	...	3·33
9,500	...	30,200	...	11·0	...	1650	...	3·18
9,780	...	30,680	...	13·5	...	1660	...	3·14
10,360	...	30,830	..	16·2	...	1630	...	2·98
10,840	...	31,370	...	21·6	...	1630	...	2·89
11,180	...	31,560	...	26·8	...	1620	...	2·82

Cast-iron

Field round iron neck per sq. cm.		Magnetic induction in the metal		Current in field magnets, amperes		$\frac{\mathfrak{B}-\text{outside field}}{4\pi}$		$\frac{\mathfrak{B}}{\text{outside field}}$
3,900	...	19,660	...	1·97	...	1250	...	5·04
6,400	...	21,930	...	3·75	...	1240	...	3·42
7,710	...	22,830	...	5·38	...	1200	...	2·96
8,080	...	23,520	...	7·08	...	1230	...	2·91
9,210	...	24,580	...	13·15	...	1220	...	2·67
9,700	...	24,900	...	16·9	...	1210	...	2·57
10,610	...	25,600	...	22·6	...	1190	...	2·46

The magnetic force within the metal ($\mathfrak{H}$) differs from the field in the surrounding space by an amount which cannot be estimated without a knowledge of the distribution of free magnetism on the pole-pieces and conical faces of the bobbin. It appears probable that, with the dimensions of the various parts used in these experiments, the magnetic force within the metal is less, but not very greatly less, than the outside and closely neighbouring field. In the absence of any exact knowledge of $\mathfrak{H}$, it is interesting to examine the relation of $\mathfrak{B}$ to the outside field. Thus, ($\mathfrak{B}$ − outside field)/4π gives a quantity which is probably not much less than the intensity of magnetism, $\mathfrak{I}$. The values of this quantity and also of the ratio $\mathfrak{B}$/outside field for Lowmoor wrought-iron and cast-iron are stated in the tables above.

Curves are given showing the relation of $\mathfrak{B}$ to $\mathfrak{B}$/outside field for Lowmoor iron and for cast-iron, in the manner introduced by Rowland for showing the relation of $\mathfrak{B}$ to μ (the permeability). The curves have the same kind of inflection that a curve of μ and $\mathfrak{B}$ begins to have when the magnetising force is raised sufficiently high. The range through which the permeability of iron may vary is well shown by comparing the values reached here (probably in the extreme case less than 3) with the value 20,000, which was found by one of the authors in the case of a soft wire exposed to a very small magnetising force and kept at the same time in a state of mechanical vibration.

The experiments give no support to the suggestion that there is a maximum of the induction, $\mathfrak{B}$. The value of $\mathfrak{B}$ capable of being reached by the method here employed depends mainly on the scale of the experiments. Larger field magnets with pole-pieces tapering to a narrow neck should yield values of $\mathfrak{B}$ greatly in excess even of those that were observed.

Linnean Society, April 7.—Mr. William Carruthers, F.R.S., President, in the chair.—Mr. Hunter J. Barron, Mr. Jas. H. Dugdale, and Mr. Edwd. B. Poulton were elected Fellows of the Society.—Fresh specimens were exhibited of a pure white variety of primrose, which had been gathered, growing wild, near Biarritz, France, by Mr. W. D. Godolphin Osborne.—A large series of instantaneous photographs of storks nesting, &c., were exhibited for Mr. E. Bidwell. These had been taken in Germany, and were specially interesting as showing the peculiar attitudes assumed in flight, &c.—Some malformed trout in an early stage of development were shown and commented on by Dr. F. Day.—A paper was read by Prof. Huxley on "The Gentians; Notes and Queries." Taking the flower as a basis, he divides the Gentianeæ into two great series, each of which is characterised by a peculiar disposition of the nectarial organs, and a gradation of forms of the corolla from the deeply cleft rotate or stellate condition, through the campanulate to the extreme infundibulate kind. In one series termed (I.) Perimelitæ, the nectarial cells are aggregated in a single or two patches; in the other series (II.) Mesomelitæ, the distinguishing characters are a zone of secreting cells encircling the ovary, or absence of such, with presence of a honey-secreting surface which may exist in the central parts of the flower. He assumes, on morphological grounds, a hypothetical ancestral flower or Ur-Gentian = Haplanthe. As a starting-point, this would lead, on the one hand, to the series of Perimelitaæ, with four subsidiary types; and on the other to the Mesomelitæ, also with four subsidiary types of floral structure. The Perimelitæ comprise the groups:—1. Actinanthe, 2. Keratanthe, 3. Lophanthe, and 4. Stephananthe; the Mesomelitæ comprise:—1. Asteranthe, 2. Limnanthe, 3. Lissanthe, and 4. Ptychanthe. The one series appears to bear a certain progressive relation in its evolution to the leading morphological modifications of the opposite series. In treating of the geographical distribution of the gentians, Prof. Huxley adopts the lines previously followed by him on animal distribution. Under (I.) Arctogea he includes Europe, Africa, Asia, and North America as far as Mexico; South Africa, Madagascar, Hindostan, and Indo-China forming a sub-province = South Arctogea; the remainder = North Arctogea. (II.) Austro-Columbia comprises South America, isthmus and islands as far north as Mexico. (III.) Australia is another province; and (IV.) New Zealand with adjoining islands. Species of the Gentianeæ are found in all these provinces, the head-quarters being North Arctogea and Austro-Columbia. The Ptychanthe are predominant in North Arctogea; the Lissanthe in South Arctogea; and Actinanthe, Lophanthe, and Lissanthe in Austro-Columbia. In Australia and New Zealand there is a paucity of species. He considers that the present distribution of the Gentianeæ is not to be accounted for by migration from any given centre, whence diffusion to their present localities. Borrowing analogy from zoological distribution, he likens the gentians to the tapirs, at present only represented in South America and the Indo-Malayan region. Yet the Tapiridæ in the Middle Tertiary epoch were distributed everywhere in the intermediate areas. Though fossil remains of gentians are not yet known, Prof. Huxley nevertheless suggests that in Pliocene and Miocene times their distribution may have been substantially similar to what is now extant. He further throws out the hint that, could the age of the first appearance of dipterous, hymenopterous, and lepidopterous insects provided with long haustra be indicated, we should then be in a position to guess approximately when specialisation of the types of the gentians and their ultimate distribution occurred.

Geological Society, April 6.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the rocks of the Malvern Hills, part 2, by Mr. Frank Rutley. The details of the microscopic examination of the rocks constituted the principal part of the present paper. The author concluded that the rocks of the Malvern Hills represent part of an old district consisting of plutonic and, possibly, of volcanic rocks associated with tuffs, sedimentary rocks, composed mainly or wholly of eruptive materials, and grits and sandstones; that the structural planes in these rocks (sometimes certainly, at others possibly) indicate planes of stratification, and that the foliation, in many cases if not in all, denotes lamination due to deposition either in water or on land surfaces, probably more or less accentuated or altered by the movements which produced the upheavals, subsidences, and flexures prevalent in the range.—On the alleged conversion of crystalline schists into igneous rocks in County Galway, by Dr. C. Callaway.—A preliminary inquiry into the genesis of the crystalline schists of the Malvern Hills, by Dr. C. Callaway. The author's researches amongst the crystalline rocks of Connaught had suggested certain lines of investigation which had subsequently been followed out in the Malvern district. He had satisfied himself that many of the Malvern schists had been formed out of igneous rocks; but at present he limited himself to certain varieties. The materials from which these schists were produced were diorite (several varieties), granite, and felsite. The metamorphism had been brought about by lateral pressure. Evidence of this was seen in the intense contortion of granite-veins and in the effects of crushing as observed under the microscope. The products of the metamorphism were divided into two groups:—(1) Simple schists, or those formed from one kind of rock. (2) Injection schists, formed by the intrusion of veins, which had acquired parallelism by pressure. Veins of diorite in diorite produced *duplex diorite-gneiss*, and veins of granite in diorite originated *granite diorite-gneiss*. It was further noted that (1) generally the particular varieties of schist occurred in the vicinity of the igneous masses to which they were most nearly related in mineral composition; (2) the mineral banding of the rocks in the field was more like vein-structure than stratification. The author accepted the received view of the age of the schists. The parallel structure was clearly antecedent to the Cambrian epoch, and the occurrence of similar rocks as fragments in the Uriconian conglomerate of Shropshire seemed to indicate that the Malvernian schists were older Archæan. The reading of this paper was followed by a discussion in which the President, Mr. Teall, Dr. Hicks, Colonel McMahon, Dr. Callaway, and Mr. Rutley took part.

Entomological Society, April 6.—Dr. David Sharp, President, in the chair.—Mr. Francis Galton, F.R.S., Mr. J. H. Leech, B.A., F.L.S., and Mr. G. S. Parkinson, were elected Fellows.—Mr. S. Stevens exhibited specimens of *Arctia mendica*, collected in the county of Cork. The peculiarity of the Cork form of the species is that the majority of the males are as white as the female of the English form, and the typical black or English form appears to be unknown in Cork.—Mr. McLachlan exhibited a zinc box used by anglers for the purpose of keeping living flies in, which he thought might be adapted to practical use in the field by entomologists.—Mr. G. T. Porritt exhibited specimens of *Hybernia progemmaria*, from Huddersfield. All the females and a large proportion of the males were of the dark variety *fuscata*, which formerly was almost unknown in Yorkshire, but which now seemed likely to replace the original type. Mr. Jenner-Weir and Lord Walsingham both remarked that the number of melanic forms appeared to be on the increase in the north, and suggested explanations of the probable causes of such increase.—Mr. Gervase F. Mathew, R.N., exhibited several new species of Rhopalocera taken by him in the Solomon Islands during the visits to those islands of H.M.S. *Espiègle* in 1882 and 1883. Amongst them were species of *Euplœa*, *Mycalesis*, *Messarus*, *Rhinopalpa*, *Cyrestis*, *Diadema*, *Parthenos*, *Pieris*, *Papilio*, &c.—Mr. E. B. Poulton exhibited a large and hairy lepidopterous larva brought from Celebes by Dr. Hickson, and made remarks on the urticating properties of the hairs of the species, which were said by the natives to produce symptoms similar to those of erysipelas if the larva was handled. Lord Walsingham, Mr. McLachlan, Dr. F. A. Dixey, Mr. Jenner-Weir, Dr. Sharp, Mr. Slater, and Mr. Poulton, took part in a discussion as to whether urtication was due to the mechanical action of the hairs in the skin, or to the presence of formic acid, or some other irritant poison, in glands at the base of the hairs. There appeared to be no doubt that in some species the irritation caused by handling them was merely due to the mechanical action of the hairs.—Mr. P. Crowley exhibited a collection of Lepidoptera recently received from West Africa, including several new or undescribed species of *Mylothris*, *Diadema*, *Harma*, *Rhomaleo-*

soma, &c.—Mr. H. Goss announced the capture by Mr. G. D. Tait, at Oporto, of a specimen of *Anosia plexippus*, and remarked that, although some twenty specimens had been caught in the South of England, only two specimens had been previously recorded from the continent of Europe.—Lord Walsingham read a paper entitled "A Revision of the Genera *Acrolophus* (Poey) and *Anaphora* (Clem.)"; and exhibited about twenty new species of these and allied genera. Mr. Stainton made some remarks on the genera, and said he was glad Lord Walsingham was working at them and their allies.—Mr. Poulton read "Notes in 1886 on Lepidopterous Larvæ." In the discussion which ensued, Lord Walsingham referred at some length to instances of protective resemblance in larvæ, and alluded to the existence in certain species, especially of the genus *Melitæa*, of prothoracic glands.—Dr. F. A. Dixey remarked on the extraordinary powers of contraction which appeared to be possessed by the retractor muscle of the flagellum in *D. vinula*, and inquired whether any corresponding peculiarities of minute structure had been observed in it. The discussion was continued by Mr. G. F. Mathew, Mr. W. White, Dr. Sharp, Mr. Porritt, and others.

Paris

Academy of Sciences, April 18.—M. Janssen, President, in the chair.—On an absolute unity of time; electric standards of time and chronoscopes of their variations, by M. Lippmann. It is shown that from the study of certain electric phenomena an absolute invariable unity of time may be obtained. The electric apparatus here described yields more accurate results than the best constructed astronomical clock. It has the further advantage of indicating, recording, and, where needed, automatically correcting, its variations of velocity.—Solar observations made at Rome during the first quarter of the year 1887, by M. Tacchini. The period of minimum activity for the spots and faculæ, as recorded in November 1886, has continued throughout the first three months of 1887. The protuberances also continued to decline during the same period.—On antipyrine, an antidote against pain, by M. Germain Sée. Antipyrine, with the formula $C_{11}A_{12}N_2O$, discovered by Knorr in 1884, is shown to be not only a good febrifuge, but also a most efficacious remedy for gout, rheumatism, and similar affections.—On the earthquake of February 23, by M. J. L. Soret. Certain derangements of the telephonic apparatus in the central office at Cannes seem to show that the violent shock which occurred at 5.50 a.m. was accompanied by strong electric discharges.—On a special circumstance connected with the production of the bicarbonate of soda, by M. Paul de Mondésir. An experiment here described shows that the carbonate of soda, combined with a single equivalent of water, scarcely absorbs carbonic acid at the ordinary temperature at all. But when it is mixed with a slight portion of dry bicarbonate the reaction on the contrary begins at once, and with an energy in direct proportion to the quantity of the bicarbonate and the thoroughness of the mixture.—Method of determining the relative value of the four unities of chemical action of the atom of carbon, by M. Louis Henry. At the base of the doctrines of organic chemistry as now understood lie the two principles of the quadrivalence of the atom of carbon, and the identity in value of its four unities of chemical action. The former is an accepted fact proved by experiment, while the latter is far from possessing the same degree of objective certainty. The author here proposes a method by means of which its truth may be rigorously determined.—Artificial reproduction of rose-red spinel (balas ruby), by M. Stanislas Meunier. The author, who has been for several years incidentally occupied with the synthesis of spinel, has lately again attacked the problem in a way quite different from his first method. He now crystallises the aluminate of magnesia, tinted a rose colour by traces of chromium, the result being a stone absolutely identical with the balas ruby of nature. The experiment here described has already been varied in several ways, yielding a series of products, such as the aluminates of zinc, iron, &c., besides secondary compounds, whose study is still in progress.—On the functions of the semicircular ducts, by M. J. Steiner. From experiments made on Crustaceans at the Zoological Station at Naples, the author shows that these organs exercise little or no influence on the locomotion of the lower animals, as supposed by Delage, Viguer, and others.—On a station of the Stone Age discovered at Chaville, by M. E. Emile Rivière. This station of Chaville lies on the skirt of the wood of like name on the right side of the road between Paris and Versailles. Here the author has just discovered a large number of flint implements of the Neolithic epoch, scrapers, knives, arrow-heads, and the like. All lay on or near the surface, and were of a more or less deep gray colour, some showing clear traces of the action of fire. They closely resemble the objects found by the author at the Neolithic station of Trou-au-Loup, Clamart, in 1884-85. Amongst them was a small fragment of black, siliceous pottery without any ornamentation, and also perfectly analogous to the pottery of the Neolithic beds in the same neighbourhood.—The members of the International Conference on Celestial Photography were present at the sitting, and were welcomed by the President in an appropriate address dwelling especially on the importance for astronomy of the photographic labours of MM. Henry, of Paris.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

Hyderabad, Kashmir, Sikkim, and Nepal: Sir R. Temple and Capt. R. C. Temple (Allen).—Geology of England and Wales, 2nd edition: H. B. Woodward (Philip).—An Elementary Treatise on the Mathematical Theory of Perfectly Elastic Solids: W. J. Ibbetson (Macmillan).—Dynamics for Beginners; Rev. J. B. Lock (Macmillan).—Catalogue of Lizards, and edition, vol. iii.: G. A. Boulenger (British Museum).—Mineral Resources of the United States, 1885 (Washington).—Photography of Bacteria: E. M. Crookshank (Lewis).—Chemistry for Beginners: R. L. Taylor (Low).—Woodland Tales: J. Stinde (Unwin).—Geological and Natural History Survey of Minnesota, 13th and 14th Annual Reports: W. H. Winchell (St. Paul).—Longmans' New Geographical Readers, Standard VII. (Longmans).—The Prevention of Consumption: C. Candler (K. Paul).—Pictographs of the North American Indians: G. Mallery (Washington).—Der Bau des Menschen: Dr. R. Wiedersheim (Williams and Norgate).—Thomas Young: Prof. Tyndall.—Oberpliocän-Flora aus den Baugruben des Klärbeckens bei Niederrad und der Schleuse bei Höchst A. M.: Drs. Geyler and Kinkelin (Frankfort).—The Eruption of Krakatão: E. D. Archibald.—City Government of St. Louis: M. S. Snow (Baltimore).—Notes to Accompany a Geological Map of Northern Canada: G. M. Dawson (Montreal).—Étude Numérique des Concours de Compensation de Chronomètres: G. Cellérier (Genève).—Journal of the Royal Statistical Society, March (Stanford).—Zeitschrift für Wissenschaftliche Zoologie, xlv. Band. 2 Heft (Leipzig).—Bulletin de l'Académie Royale des Sciences de Belgique, No. 3 (Bruxelles).

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